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PHILOSOPHICAL

TRANSACTIONS.

IX. *Abstract of Observations on a diurnal Variation of the Barometer between the Tropics. By* J. Horsburgh, *Esq. In a Letter to* Henry Cavendish, *Esq. F. R. S.*

Read March 14, 1805.

SIR, Bombay, April 20, 1804.

When I was in London at the conclusion of the year 1801, I had the pleasure of being introduced to you by my friend Mr. Dalrymple, at which time he presented you with some sheets of meteorological observations, with barometer and thermometer, made by me in India, and during a passage from India to England.

Being of opinion that few registers of the barometer are kept at sea, especially in low latitudes, I have been induced to continue my observations since I left England, judging that, even if they were found to be of no utility, they might at least be entertaining to you or other gentlemen, who have been making observations of a similar nature.

During my last voyage I have employed two marine

barometers, one made by TROUGHTON, the other by RAMSDEN, and a thermometer by FRAZER. These were placed exposed to a free current of air in a cabin, where the basons of the barometers were 13 feet above the level of the sea.

The hours at which the heights of the barometers, and thermometers were taken, *viz.* noon, IV hours, X hours, XII hours, XVI hours, and XIX hours, were chosen, because at these times the mercury in the barometer had been perceived to be regularly stationary between the tropics, by former observations made in India in 1800 and 1801. It was found that in settled weather in the Indian seas, from 8 AM to noon, the mercury in the barometer was generally stationary, and at the point of greatest elevation; after noon it began to fall, and continued falling till 4 afternoon, at which time it arrived at the lowest point of depression. From IV or V PM the mercury rose again, and continued rising till about IX or X PM, at which time it had again acquired its greatest point of elevation, and continued stationary nearly till midnight; after which it began to fall, till at IV AM it was again as low as it had been at IV afternoon preceding; but from this time it rose till 7 or 8 o'clock, when it reached the highest point of elevation, and continued stationary till noon.

Thus was the mercury observed to be subject to a regular elevation and depression twice in every 24 hours in settled weather; and the lowest station was observed to be at about 4 o'clock in the morning and evening. I remarked that the mercury never remained long fixed at this low station, but had a regular tendency to rise from thence till towards 8 in the morning and about 9 in the evening, and from those times continued stationary till noon and midnight.

In unsettled blowing weather, especially at Bombay during the rains, these regular ebbings and flowings of the mercury could not be perceived; but a tendency to them was at sometimes observable when the weather was more settled.

In the sheets, which I formerly presented to you, were evinced these elevations and depressions twice every 24 hours within the tropics, in steady weather, as had been observed by Mess. Cassan and Peyrouse, by Dr. Balfour of Calcutta, and others. But since my last arrival in India, I have observed that the atmosphere appears to produce a different effect on the barometer at *sea* from what it does on *shore*.

As I am ignorant whether this phenomenon has been noticed by any person before, I will here give you an abstract of my journal, shewing how the barometer has been influenced during the whole time since I left England, which will enable you to form an idea whether I am right in concluding that the barometer is really differently affected at sea from what it is on shore, at those places in India where the observations have been made.

The first sheet begins with the observations made on board ship, in my voyage from London towards Bombay, in the months of April and May, 1803.

From the time of leaving the Land's End, April 19th, the motion of the mercury in barometers was fluctuating and irregular until we were in latitude 26° N, longitude 20° W, on April 29th; the mercury in barometers then became uniform in performing two elevations and two depressions every 24 hours, (which for brevity in mentioning hereafter I will call equatropical motions.) From latitude 26° N to latitude 10° N, the difference of the high and low stations of the mercury in the

barometers was not so great, as it was from latitude 10° N across the equator, and from thence to latitude 25° S. Within these last-mentioned limits, the difference of high and low stations of the mercury in the barometers was very considerable, generally from five to nine hundered parts of an inch, both in the daily and nightly motions.

When we reached the latitude of 28° S, longitude 27° W, June 7th, the mercury in barometers no longer adhered to the equatropical motions; but then, as in high north latitudes, its rising and falling became irregular and fluctuating during our run from latitude 28° S, longitude 27° W, (mostly between the parallels of 35° and 36° S,) until we were in latitude 27° S, and longitude 51° E, on the 11th of July. The mercury then began to perform the equatropical motions, and continued them uniformly, during our run from the last-mentioned position, up the Madagascar Archipelago, across the Equator, until our arrival at Bombay July 31st, 1802.

August 6th, 1802. When the barometers were placed on shore in Bombay, the mercury, for the first six days, appeared to have a small tendency towards performing the equatropical motions, but not equally perceptible as when at sea, the difference between the high and low stations of the mercury in the barometers being great to the day we entered the harbour of Bombay. From the 12th of August to the 22d the mercury could not in general be observed to have any inclination to perform the equatropical motions, although at times a very small tendency towards performing them might be perceived.

On the 23d of August the barometers were taken from the shore to the ship. Immediately on leaving Bombay harbour, August 26th, 1802, the mercury in the barometers performed

the equatropical motions, and continued them with great uniformity, during our passage down the Malabar coast, across the bay of Bengal, in the Strait of Malacca, and through the China Sea, until our arrival in Canton river on the 4th of October. When in the river, the mercury became nearly stationary during the 24 hours, except a small inclination at times towards the equatropical motions, but they were not near so perceptible as at sea; this change taking place the day we got into the river.

During our stay in China, the barometer on shore, at Canton, had very little tendency towards the equatropical motions, throughout the months of October and November that we remained there. At times, while in China, a small inclination towards performing the equatropical motions appeared: but, as in Bombay, the difference of rise and fall was of so small a quantity, as to be frequently imperceptible.

December 2d, 1802. On our departure from Canton river, the equatropical motions were instantly performed by the mercury, and with great regularity continued during the whole of the passage to Bombay, until our arrival in that harbour on the 11th of January, 1803.

On January 18th, the barometers were placed on shore, and did not appear in the smallest degree subject to the equatropical motions; although, with great regularity, they had been performed while at sea, even to the day we entered the harbour. One of the barometers was left on board for a few days, and, like that on shore, seemed to have no tendency towards the equatropical motions. During the months of February and March, in Bombay, the mercury was nearly stationary throughout the 24 hours. But about the latter part of March

the mercury seemed to incline towards the equatropical motions in a very small degree; and, during the month of April, and to the 20th of May, this small tendency of the mercury to perform the motions appeared at times, but was hardly discernible, the rise and fall being of so small a quantity. From the 18th of January to the 20th of May, the mercury in the barometers was in general stationary, except a very small tendency towards the equatropical motions at times. At other times some change in the atmosphere disturbed the mercury from its stationary position; but this was seldom the case, as it was then the fair weather season, or north-east monsoon.

We sailed from Bombay on the 23d of May, 1803. The instant we got out of the harbour, the mercury in the barometers conformed to the equatropical motions with great regularity, and the difference between the high and low stations was very considerable during the whole of the passage to China, excepting a few days in the eastern parts of Malacca Strait, where the land lay contiguous on each side of us; the difference between the high and low stations of the mercury was then not so great as in the open sea. On clearing the Strait, and entering the China Sea, the equatropical motions were performed in greater quantity, and continued regular during our passage up the China Sea, until July 2d, 1803. We then entered Canton river, and the equatropical motions of the mercury in barometers entirely ceased.

From July 8th to September 7th, the barometers were placed on shore in Canton, during which time the mercury appeared to have no tendency towards performing the equatropical motions; but it inclined to a stationary position, except when influenced by changes of weather. After the barometers were

taken from Canton to the ship, we were four days in getting clear of the river, in which time the mercury inclined to be stationary, excepting that a small inclination towards the equatropical motions seemed to evince itself at times. But no sooner had we cleared Canton river, September 13th, 1803, than the mercury in the barometers began to conform to the equatropical motions, of two elevations and two depressions every 24 hours, at equal intervals of time, (although we were near the land until the 15th September.) And the mercury, with great regularity, continued to perform the equatropical motions, from September 13th, 1803, the day we cleared the river of Canton, until October 13th, when we entered Sincapore Strait, excepting a small degree of irregularity, which affected the mercury on the 22d September, when it blew a gale on the coast of Isiompa.

October 13th, 1803. On entering the Strait of Sincapore, which is about $3\frac{1}{2}$ leagues wide, the mercury in the barometers was then a little obstructed, and did not perform the equatropical motions, in the same quantity of rise and fall, as when we were in the China Sea. But on the following day, October 14th, when we had passed the narrow part of the Strait, the mercury conformed to those motions with regularity until October 21st, when we arrived in the harbour of Prince of Wales's Island; then a great retardation took place in the equatropical motions; for, during the time the ship remained in the harbour, from October 20th to November 5th, 1803, the mercury in barometers seemed only in a small degree subject to them, the difference between the high and low stations of the mercury, being in general not more than half the quantity, that takes place in the open sea, or at a considerable distance

from land. Where the ship lay at this time in the harbour, the land, on one side, was a full quarter of a mile distant, and on the other side about 1½ mile.

On November 5th, being clear of the harbour of Prince of Wales's Island, the equatropical motions were instantly performed by the mercury, in the usual quantity experienced at sea, which continued with uniformity until December 3d. On this, and the following day, the mercury fell considerably during our passage over the tails of the sands at the entrance of Hoogly river, in latitude 21° 06′ N; and on December 5th, the day of the moon's last quarter, a gale of wind commenced from NNE, with much lightning and rain in the night. During the latter part of this day, the mercury began to rise, and there soon followed a change of settled weather. When we were in the lower part of the river, the mercury appeared to conform in a small degree to the equatropical motions; but when well up the river, at Diamond Harbour, the mercury inclined to be nearly stationary during the 24 hours, as has formerly been observed to happen in Canton river, Bombay harbour, &c.

On January 13th, 1804, after we had cleared the river Hoogly, the mercury in the barometers began to perform its motions with uniformity, which continued during the passage to Bombay, until our arrival there on February 12th. The barometers being then placed on shore, the mercury inclined to a stationary position, without evincing any propensity towards the equatropical motions from the 12th to the 18th February, 1804, as has been noticed in the foregoing description, to happen frequently, on entering a harbour from sea.

On February 18th, 1804. The meteorological journal ceases,

at which time it comprises the observations of 22 months, having commenced April 6th, 1802, in Margate Road.

I have taken the liberty of sending you this abstract from the journal, to exhibit the apparent difference of the mercury in the barometer at sea, from what has been observed on shore, at those places mentioned in the preceding description. As I have not seen any account indicating the phenomenon, I thought it might be interesting to you, or other gentlemen of the Royal Society to forward this imperfect abstract, the journal itself being too cumbersome to send home at present. But as I am in expectation of returning to England by the ships from China next season, I hope I shall be enabled to present you with the meteorological sheets alluded to above.

I am, &c.

J. HORSBURGH.

P. S. Since I wrote the foregoing abstract, I have received a letter from my friend Mr. DALRYMPLE, intimating that a copy of the meteorological journal itself would be acceptable, which has induced me to transmit to him the original sheets, with a request to deliver them to you. I regret that I could not find leisure time to make out a fair copy, to have sent to you, in place of the original sheets in their rough state.

Bombay,
June 1st, 1804.

X. *Concerning the Differences in the magnetic Needle, on Board the Investigator, arising from an Alteration in the Direction of the Ship's Head.* By Matthew Flinders, *Esq. Commander of his Majesty's Ship Investigator. In a Letter to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read March 28, 1805.

WHILST surveying along the south coast of New Holland, in 1801 and 1802, I observed a considerable difference in the direction of the magnetic needle, when there was no other apparent cause for it than that of the ship's head being in a different direction. This occasioned much perplexity in laying down the bearings, and in allowing a proper variation upon them, and put me under the necessity of endeavouring to find out some method of correcting or allowing for these differences; for unless this could be done, many errors must unavoidably get admission into the chart. I first removed two guns into the hold, which had stood near the compasses, and afterwards fixed the surveying compass exactly a-midships upon the binnacle, for at first it was occasionally shifted to the weather side as the ship went about; but neither of these two arrangements produced any material effect in remedying the disagreements.

The following Table contains the observations for the variation of the compass in which the differences are most remarkable, and from which I shall beg to point out such inferences as I think may be drawn from them.

Time.	Latitude.	Longitude.	Number of compasses used.	Number or sort of observations taken.	Place of the observation.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1801.	° ′	° ′				° ′	° ′		
Dec. AM	35 5 S	116 28 E	two	4 azimuths	binnacle	7 0 W	5 59 W	NW b. N	Commander
	Princess Royal Harbour		three	6 —	on shore	6 15	6 23	—	
	—	—	theodolite	1 —	—	—	6 8	—	
1802.									
Jan. 9, PM	34 1	121 40	one	2 —	binnacle	5 0	9 22	ESE	
16, AM	Goose Island Bay		one	2 —	—	4 0	0 54	W	
1803.									
May 20, AM	—	—	—	2 —	—	—	6 8	E	
1802.									
Jan. 18, PM	33 37	124 10	—	2 —	—	4 30	5 44	NNE	
20, PM	32 38	125 35	—	2 —	—	—	7 15	E b. N	
21, PM	32 30	125 48	—	2 —	—	—	4 45	S	
22, PM	32 30	126 7	—	2 —	—	4 15	6 13	NE b. E	
23, PM	32 21	126 33	—	1 amplitude	—	4 0	4 18	S b. E	
24, AM	32 5	128 15	—	2 azimuths	—	3 0	6 4	E b. N	
26, PM	32 25	128 15	three	6 —	—	—	3 17	S b. E	Lt. Flinders
30, AM	32 18	132 29	one	2 —	—	0 30	1 41	SSE	Commander
Feb. 4, AM	No. 4, Bay in Island c		—	2 —	—	0 15	2 23	Easterly	
5, AM	32 39	133 55	—	1 amplitude	—	—	1 56	E b. S	
6, AM	32 36	133 58	three	6 azimuths	—	—	1 0 E	NW	Lt. Flinders
16, PM	34 3	135 20	one	2 —	—	1 5 E	1 33 W	SE b. E	Commander
—, PM	34 5	135 24	—	1 amplitude	—	1 5	3 56 E	SW	
18, AM	34 50	135 32	three	6 azimuths	—	1 12	2 22	S	Lt. Flinders
Mar. 1, PM	In No. 10,	bay	theodolite	1 —	on shore	1 39	1 39	—	Commander
5, PM	—	—	one	2 —	binnacle	1 39	0 53	S b. E	
17, PM	34 12	137 20	—	1 amplitude	—	2 15	4 38	SW b. S	Commander
18, PM	34 23	137 36	one	2 azimuths	binnacle	2 15	0 35	SE	
21, AM	35 33	137 15	three	6 —	—	2 40 E	1 10	SE b. S	
23, AM	Kanguroo	Island	two	4 —	—	2 58	6 31	SSW	
26, AM	35 10	137 41	one	1 amplitude	—	2 45	1 49	NE b. N	
27, AM	35 21	137 52	one	1 —	—	2 50	1 49	SSE	
April 6, AM	Kanguroo	Island	—	2 azimuths	on shore	2 58	2 58	—	
10, AM	35 47	139 15	—	1 amplitude	binnacle	3 0	5 11	W b. S	
11, PM	35 53	139 26	—	2 azimuths	—	3 0	0 50	SE	
13, PM	36 45	140 5	—	2 —	—	3 30	1 25	SE b. S	
16, AM	37 55	139 55	—	2 — / 1 amplitude	—	4 15	2 20	—	
17, PM	37 57	139 56	—	2 azimuths	—	4 15	2 2	NE	
22, AM	39 58	144 50	—	2 —	—	7 45	13 52	WSW	
26, PM	38 35	144 25	—	2 —	—	7 30	3 41	NE b. E	
—, AM	38 38	144 35	—	1 amplitude	—	7 30	6 48	NE b. N	

Note. All the compasses made use of on board the Investigator were of WALKER's construction, one excepted, which was made by ADAMS, and used only on July 22d, 1801.

It is apparent that some of the observed variations in the above Table are 4° less and others 4° greater than the truth; and it may be remarked, that when this error is westward, the ship's head was east, or nearly so, and when it was eastward the head was in the opposite direction. When the observations agree nearest with what was taken on shore, or with what may be deemed the true variation, the ship's head was nearly north or south; and a minute inspection of the Table will favour the opinion, that the excess or diminution of the variation was generally in proportion as the ship's head inclined on either side from the magnetic meridian.

After I had well ascertained the certainty of a difference in the compasses, arising from an alteration in the point steered, I judged it necessary, when I wanted a set of bearings from a point where we tacked the ship, to take one set just before and another immediately after that operation: some specimens of these here follow.

		Head ESE.	*Head* SW b. W.
1802. April 13th 11h 32′, AM	Le Geographe Rocks.	N 55° to 71° E	
	π point	N 4 W	after tacking N 9° W
—	Π point	S 32 E	S 40 E.
		Head SE b. E.	*Head* W.
April 14th 9h 29′, AM	Π point rocky, inner part	N 39° E	after tacking, N 30° E
	——— projecting part	N 67 E	N 59 E
—	Furthest visible extreme from deck	S 51 E	S 55 E.
		Head ENE.	*Head* SW b. S.
April 15th 11h 50′, AM	Π, the western part	N 15° W	after tacking, N 21° W
	A peaked hummock	N 19 E	N 15 E
—	Furthest extreme from deck	S 53 E	S 61 E
—	Centre of a naked sandy patch	E	E 5 N.
Variation per amplitude April 15, AM, taken with the surveying compass		4° 8′ E, ship's head being S.	
		Head E.	*Head* SW b. S.
April 15th, 5h PM	The peaked hummock	N 12° W	after tacking, N 18° W
	Former extreme, a projection	S 59 E	S 64 E
—	Naked sandy patch, distant 3¼′	N 33 E	N 31 E.

From some little change of place after tacking the ship, and from the part whose bearing was set not being perhaps the individual spot in both instances, the difference between the separate bearings in any set will not be always the same: to these causes for error also may be added inaccuracies in taking the angles arising from the motion of the ship and compass, from the view of the object being obstructed by the rigging, masts, or ship's upper works, and from too much haste to get the bearings before the ship's place was materially altered. Even in the Table of azimuths and amplitudes greater accuracy than one degree must not be looked for; and in ship-bearings 2 or even 3 degrees is not, I believe, too great an allowance for error, unless in very favourable circumstances.

Without attending to small differences, it is evident that the bearings correspond with the observations in requiring a less east variation to be applied when the ship's head was easterly, and a greater when it was to the westward, in order to get at the true direction of the object.* When examining the north

* As a specimen of the plan I followed in protracting such bearings as the above, take the set of April 15, AM, when the true variation appears to have been 4° E. On the first bearings the ship's head was six points on one side of the meridian, and on the second it was three points on the other side, the mean is one point and an half on the east side: now for this one point and an half I allow 1° of error, which, as it is on the east side of the meridian, and the variation is easterly, must be subtracted: the variation then to be allowed upon the mean between the bearings before and after tacking will be 3° E, from which the true bearings will stand as follows:

April 15th, AM	North western part	N 15° E
11h 50′	A peaked hummock	N 20 E
—	Furthest extreme from deck	S 54 E
—	Centre of a naked sandy patch	E 0¼ S.

In the same manner upon single sets of bearings I was obliged to allow a variation different from what I supposed the true to be, unless the ship's head was nearly north or south: but, that I might proceed as little upon conjecture as possible, I always

and east coasts of New Holland, I always endeavoured to take the angles on shore with a TROUGHTON's portable theodolite, and to observe for the variation in the same places, that all the errors might be done away or corrected; and as I was frequently fortunate enough to carry on my surveys in this manner for weeks together, instances that might corroborate or contradict the preceding remarks are neither very numerous or pointed; the following are the most remarkable.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1802.	° ′	° ′				° ′	° ′		
Aug. 5, PM	23 51 S	151 42 E	one	1 amplitude	binnacle	8 0 E	7 2 E	WSW	Commander
— AM	23 51	151 40	—	—	—	—	10 15	WNW	
12, PM	23 30	151 11	three	6 azimuths	—	—	6 50	SSE	Lt. Flinders
18, PM	23 23	151 16	one	2 —	—	7 45	7 52	W	Commander
31	22 23	150 38	two	4 —	—	7 30	4 49	E	
Sept. 6, AM	Upon Pier Head		theodolite	1 —	on shore	8 0	8 2	—	
Oct. 14, PM	20 44	150 42	one	1 amplitude	binnacle	7 0	6 40	SSE	Lt. Flinders
20, PM	19 22	148 40	—	1 —	—	6 0	5 39	S	Commander
21, AM	18 15	148 38	three	6 azimuths	—	—	5 42	N b. E	Lt. Flinders
Nov. 2, PM	10 30	142 32	one	2 —	—	4 0	3 32	E	Commander
7, AM	12 11	142 0	—	2 —	—	—	4 4	S	Lt. Flinders
9, PM	12 37	142 2	—	1 amplitude	—	—	5 24	W	Commander
1803.									
Jan. 3, PM	14 20	136 16	—	1 —	—	2 30	0 58	E	
7, PM	14 20	136 37	—	1 —	—	—	1 9	SE	
13. {PM / AM}	13 38	137 20	—	2 —	—	3 0	3 47	Westerly	Lt. Flinders
14, AM	13 35	136 58	—	1 —	—	—	3 51	WSW	Commander
16, PM	In NW Bay	(Gr. Eyl.)	theodolite	1 azimuth	on shore	—	3 6	—	
Feb. 3, AM	Arnhem's Bay		three	6 —	binnacle	2 20	2 26	NW b. W	
9, AM	—	—	theodolite	1 —	on shore	—	2 20	—	
Mar. 10, AM	11 5	134 15	one	2 —	binnacle	1 0	1 55	WNW	

endeavoured to get observations for the variation when the ship's head was in the same direction as when I had taken or wished to take a particular set of bearings, and I then allowed that variation exactly, whatever it was. The perplexity arising from disagreements in bearings was by these means much alleviated, and happy agreements were frequently produced, when, without such corrections, there was nothing but discord.

In the latter of these observations, the differences arising from a change in the direction of the ship's head is less considerable than in the higher latitudes; indeed, on approaching the line of no variation upon the south coast, the differences in the variation were smaller than before and afterwards; but that these differences shall be greater in a large variation and smaller in a less, both places being equally distant from the magnetic pole, I will not venture to assert. The inferences that I think may be safely drawn from the above observations are as follows. 1st. That there was a difference in the direction of the magnetic needle on board the Investigator when the ship's head pointed to the east, and when it was directed westward. 2d. That this difference was easterly when the ship's head was west, and westerly when it was east. 3d. That when the ship's head was north or south the needle took the same direction or nearly so that it would on shore; and shewed a variation from the true meridian, which was nearly the medium between what it showed when east and when west. 4th. That the error in variation was nearly proportionate to the number of points which the ship's head was from the north or south. Constant employment upon practice has not allowed me to become much acquainted with theories, but the little information I have upon the subject of magnetism has led me to form some notion concerning the cause of these differences, and although most probably vague and unscientific, I trust for the candour of the learned in submitting it, as well as the inferences above drawn, to their judgment.

1st. I suppose the attractive power of the different bodies in a ship, which are capable of affecting the compass, to be collected into something like a focal point or center of gravity,

and that this point is nearly in the center of the ship where the shot are deposited, for here the greatest quantity of iron is collected together.

2d. I suppose this point to be endued with the same kind of attraction as the pole of the hemisphere where the ship is; consequently, in New Holland the south end of the needle would be attracted by it and the north end repelled.

3d. That the attractive power of this point is sufficiently strong in a ship of war to interfere with the action of the magnetic poles upon a compass placed upon or in the binnacle.

If these suppositions are consistent with the laws of magnetism, established by experiments, I judge that they will account for all the differences above noticed; for the interference will necessarily be most perceptible upon a compass when the attractive point is at right angles to the magnetic meridian, that is, when the ship's head is east or west, and will altogether vanish or become imperceptible when the attractive point and meridian coincide, or when the ship's head is north or south. That the power of this point should become less as the ship increases her distance from the magnetic pole has not indeed entered into my suppositions; but it may probably be true, and is indeed almost a necessary consequence of the second supposition. If the above hypothesis, so to call it, be true, it must follow, that the differences in the variation of the magnetic needle, arising from a change in the ship's head, ought to be directly contrary to those before recited, when the ship is on the north side of the magnetic equator, for the north point of the needle should then be attracted, and the south end repelled. I have no observations which are very decisive upon this head, but those that were taken on board

the Investigator seem to bespeak that it is so; they are as follows.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1801.							° ′		
July 21, PM	Start Point in sight		two	5 azimuths	binnacle	—	29 34 W	W	Mr. Thistle
—	to the NE		one	1 amplitude	—	—	29 30	—	——
22, PM	49° 10′ N	5° 25′ W	two	4 azimuths	binnacle	—	24 12	WNW	——
—, AM	48 15	6 45	one	1 amplitude	upon the booms in the middle of the ship	+	20 49	WSW	——
28, PM	38 1	14 20	five	10 azimuths	binnacle	—	20 57	SW	——
—	—	—	—	11 —	binnacle	—	25 34	—	Commander
31, PM	Porto Santo in sight		two	4 —	—	—	22 45	—	Mr. Thistle
—	to the NW		—	4 —	booms	—	19 31	—	——
Aug. 24, AM	10 20	22 15	one	2 —	binnacle	—	12 45	SE b. S	Commander
29, AM	5 40	16 30	two	4 —	—	—	12 18	—	Lt. Flinders
Sept. 5, AM	2 15	14 00	—	3 —	—	—	14 54	WSW	Mr. Thistle

These observations, particularly those of July 28, seem to be decisive in showing that the variation is more westerly when taken upon the binnacle of a ship whose head is westward in north latitude, than when observed in the center of the ship, which is a strong confirmation of the suppositions before given; but the observations on the change of the ship's head are too few to be satisfactory. Almost every sea officer can tell whether he has observed the variation of the compass to be greater when going down the English Channel than when coming up it: and indeed it would be very easy for a ship lying in harbour to ascertain the point beyond controversy. Should this point be well established, I think it would follow, that from a high south latitude where the differences are great on one side, they are most likely to decrease gradually to the equator, and to increase in the same way to a high north latitude, where they are great on the other side; thus the smaller

differences on the north coast of New Holland will be accounted for. I shall leave it to the learned on the subject of magnetism to compare the observations here given with those made by others in different parts of the earth, and to form from them an hypothesis that may embrace the whole of the phenomena: the opinion I have ventured to offer is merely the vague conjecture of one who does not profess to understand the subject. Some account of the magnetism of Pier Head, upon the east coast of New Holland, may not perhaps be thought an unappropriate conclusion to this Paper. I was induced to attend to this from the following passage in HAWKESWORTH, Vol. III. p. 126. "At sun-rise I went ashore," says Captain COOK, "and "climbing a considerable hill," Pier Head, "I took a view of "the coast and the islands that lie off it, with their bearings, "having an azimuth compass with me for that purpose; but I "observed that the needle differed very considerably in its "position, even to thirty degrees, in some places more, in "others less; and once I found it differ from itself no less "than two points in the distance of fourteen feet.* I took up "some of the loose stones that lay upon the ground, and "applied them to the needle, but they produced no effect;

* In a set of angles taken near the head of Arnhem north bay, on the west side of the gulph of Carpentaria, I found the needle of the theodolite had been drawn 50° from its proper direction. The shore consisted of grains of iron ore caked into a stony mass; and a piece of it, when applied to the needle, drew it 6 or 8 degrees from its direction, but it then swung back to its error of 50° where it was stationary. In Arnhem south bay a small piece of similar stone drew the needle of the theodolite entirely round, yet the bearings taken in this place did not show any disagreement from the variation and bearings taken in the neighbouring places, where the stone did not produce any such effect. In most places on shore, where I had occasion to take angles, it was my practice to try the effect of a piece of the stone upon the theodolite, in order to detect the presence of iron ore, as well as on account of my survey. It

"and I therefore concluded that there was iron ore in the hills, "of which I had remarked other indications, both here and "in the neighbouring parts."

On landing at Pier Head I found the stones lying on the surface to be porphyry, of a dark bluish colour; but although I understand this species is usually found to possess some magnetic power, a piece did not produce any sensible effect upon the needle of the theodolite when applied to it. In the following observations the theodolite always stood about four feet from the ground, that being nearly the length of its legs. I first took an extensive set of bearings from the top of the hill, amongst which were two stations whence Pier Head had been before set. The first, called Extensive Mount, distant 34 miles, differed from its back bearing 4° 35′ to the right, and the second, island *a*, distant 29½ miles, differed 4° 45′ the same way. I now moved the theodolite three yards to the westward, and the same two objects bore 2° 10′ to the right of their back bearing; on moving it three yards to the south-eastward from the first place, they differed 2° to the left; and on moving the theodolite four yards to the northward, the same two objects bore 1° 10′ to the right of their back bearings. On the following morning I determined to try the magnetism more particularly. Taking the theodolite and dipping-needle, I landed upon the shore of the Head, whence the top of the hill bore N 50° W, about one-third of a mile. The variation of the theodolite in this place I observed to be 8° 2′ E, and the

commonly happened that no effect was apparent, but yet I could not trust implicitly to the angles, (particularly on the main land,) unless observations for the variation were taken before the instrument was moved, or I had a back bearing of some station where such observations had been made.

inclination of the south end of the dipping needle 50° 50′, the needle stood vertical when the face of the instrument was S 2° E. I then took the following bearings: Extensive Mount 108° 30′, the same exactly as by back bearing. Double Peak 148° 30′; from hence I rowed round the Head, and landed on a rock, whence the top of the hill bore SSW one-sixth of a mile; Extensive Mount bore 110° 14′, the inclination of the dipping-needle 50° 29′, and the needle stood vertical when the instrument faced S 3° E. Thus the difference was 1¼° in the horizontal, and ½° in the vertical direction of the needle. Ascending the hill, I made the following observations on the top: Extensive Mount 118° 50′, *a* island 138° 52′, Double Peak 148° 32′; the inclination of the needle was 53° 20′, and it stood vertical at S 3° E. The differences here are 5° 10′ in the horizontal, and 2° 30′ in the vertical direction, from what the needle stood at in the first morning's place. On moving ten yards SSE, the bearings were, Extensive Mount 108° 44′, Double Peak 148° 25′; the inclination was 52° 18′, and the needle was vertical when the instrument faced S 5° W. In this 4th set of observations, the horizontal direction of the needle is only a few minutes different from the first place, but the vertical direction is 1° 28′. From the top of the hill I now moved twenty yards to the north-eastward, when Extensive Mount bore 110°, Double Peak 144° 42′; the inclination of the dipping needle was now 50° 35′, and it stood vertical at S 3° W. Thus it appears that the polarity of the magnetic needle is most interrupted at the top of the hill, both according to the theodolite and dipping-needle. Whether this may arise from some particular magnetic substance lodged in the heart of the hill, or from the attractive powers of all the substances

which compose Pier Head being centered in a similar point to what I have supposed to take place with all the ferruginous bodies lodged within a ship, I shall not attempt to decide. The greater differences in the horizontal direction of the needle observed by Captain Cook, might have arisen from his using a common azimuth compass, which was probably not further elevated from the ground than to be placed on a stone.

MATTHEW FLINDERS.

Isle of France,
March 5th, 1804.

XI. *The Physiology of the Stapes, one of the Bones of the Organ of Hearing; deduced from a comparative View of its Structure, and Uses, in different Animals.* By Anthony Carlisle, *Esq.* *F. R. S.*

Read April 4, 1805.

Anatomical descriptions of the mechanism of the eye have importantly contributed to the advancement of optics, a branch of science which has conferred numerous benefits on mankind. Whether a more intimate knowledge of the structure of the organs of hearing may illustrate the doctrines of acoustics, and thus become a source of similar advantages, can only be determined by future investigations, and experiments. The following is an attempt to exhibit a part of the instrument of hearing, taken from several orders of animals, with an intention to shew the office it holds, and the relation it bears to other parts of the auditory mechanism. The minuteness of this research will not require any apology to that learned Body, who for a long series of years have witnessed the dependance of all the systems of natural knowledge on simple particulars, well chosen, and applied to the establishment of general laws.

Doubtless the whole organ of hearing is an apparatus to collect occurring sounds, and to convey them to the seat of that peculiar sensation, regulating their intensity, or facilitating their progress, according to the degree of impetus. In these respects the ear resembles the eye.

The ossicula auditûs in man, and in the mammalia, form a series of conductors, through which sounds are transmitted from the membrana tympani, into the sensitive parts of the organ. The number, forms, and relative junctions of these ossicles are various; but, in all cases, their office seems limited to the conveyance of sounds received through the medium of air; because fishes have no parts corresponding with them. In two classes of animals, the aves, and amphibia, of LINNÆUS, one bone, in the situation of the stapes, is the only ossicle of the tympanum: in all other animals it is placed next to the seat of sensible impression, and most remote from that part of the organ on which sounds first impinge.

The ossicula auditûs are formed of bone, resembling that of teeth; it is close in texture, and brittle; in the growing state, composed of a vascular pulp, the ossification of which is completed soon after birth; and, like the teeth, they cease to grow after that process is finished. The malleus and incus are hollow, and possess an internal periosteum; and the whole series is covered by a reticular membrane which has no red blood-vessels in the adult. It has been asserted by many authors that fat, or marrow, is contained in these bones, but I am induced to attribute their occasional greasy appearance to transudation from the neighbouring parts, during the stage of putrefactive maceration, seeing that all such bones when taken from recent subjects, are free from the marks of fat. Although density seems to be a requisite condition, yet it is convenient that the bones should not be massive, as their figures and relative adaptations evidently show.

The malleus is united to the membrana tympani throughout half its long diameter, by a process called manubrium; its

detached end forms a rounded enlargement, which is articulated by a sort of hinge joint to the body of the incus. These muscles are fixed to the malleus, the most powerful of which draws the manubrium, and membrana tympani perpendicularly inward; the next in strength is inserted upon a slender stem of bone which forms a right angle with the manubrium, and on the plane of the membrana tympani. The smallest muscle is fixed to the processus major, pulling the malleus backward, and pressing its head against the joint of the incus. These muscles are all restricted in their actions to the changes produceable on the membrana tympani, because the strong connections of the joints between the malleus and incus, and the incus and stapes, admit of little motion; indeed the former joint is deficient in many animals. The incus has no muscles, and forms only a passive intervention between the malleus and stapes, which last bone has a peculiar muscle appropriated to itself. Hence it appears, that the first series of ossicula auditûs has a different office from the stapes, as will be subsequently explained.

The bone, to be now particularly considered, has been called stapes, staffa, stapha, or stapeda, from its resemblance to the stirrup of a saddle. It was first observed about the middle of the sixteenth century; and PHILIP AB INGRASSIAS, REALDUS COLUMBUS, and BARTHOLOMÆUS EUSTACHIUS, have contested the honour of its discovery.

The human stapes is $\frac{1}{20}$ of an inch in height, and $\frac{3}{20}$ in width at its basis: it weighs, when dried, $\frac{1}{32}$ of a grain.

It is divided into the following parts, *viz.*

The capitulum, or articulating head, which joins the os lenticulare.

The collum, which unites the capitulum to the two crura. And

The basis, on which the expanded crura rest and terminate.

The capitulum stapedis has a shallow, concave surface, to receive the os lenticulare, or epiphysis connected to the long leg of the incus. (*Vide* Plate IV. letter *c*.) Around this joint a strong membrane is applied in the manner of a capsular ligament. The capitulum is seldom placed exactly on the top of the Gothic arch formed by the crura, and the crus immediately under the stapedeus muscle, is always the thickest, and most curved. (*Vide* letter *a*.)

The collum is hollow, being only a thin shell of bone; on its side is a small tubercle, to which the tendon of the stapedeus muscle is affixed. See letters *a* and *b*.

The crura are curved, and their interior surfaces are grooved, leaving only a thin osseus plate.

The basis is exactly adapted to the the fenestra ovalis, more properly called fenestra vestibuli by modern anatomists, and the two ends project beyond the crura. The upper surface is generally concave, the under surface slightly convex: and here a rising border marks the insertion of the membrane which connects it to the edges of the fenestra vestibuli. *Vide* letter *c*. The outline of the basis somewhat resembles a long semi-ellipsis, one side being nearly straight, and the other convex. This figure appears adapted to the expansion of the basis, without increasing the bulk of the bone, whilst it gives leverage to the muscle.

When the stapes rests on its basis, with the straight side next to the observer, if the more curved leg be toward the left, then it is the stapes of the right ear; but if on the right,

then it is the left stapes. The arch above the straight side of the basis is more rounded than that above the curved side; the latter being an intersection of two curves like the Gothic arch. I have never seen that expansion of membrane across this arch, described by DU VERNEY; and, from the great number of ears which I have attentively examined, am induced to think that a pellicle of mucilaginous fluid, which often covers the recent bone, has been mistaken for a membrane.

The stapes stands perpendicular to the plane of the membrana tympani; a plane drawn through the crura, parallel to the length of the basis, equally bisects the cavity of the tympanum.

The stapedeus muscle arises within a special cavity in the petrous portion of the temporal bone; it is a short, thick mass of red fibres, covered by fascia; and sends forth a round tendon through a small osseous aperture at the point of the pyramidal eminence, which unites to the collum stapedis in an angle of 50 degrees, toward a line drawn perpendicular to the plane of the basis, and obliquely across its convex side, in an angle of 5 degrees from the bearing of its straight side. The action of the stapedeus muscle is to draw the capitulum downward, and toward the curved side of the basis. This oblique motion depresses the end of the basis under the curved crus, whilst it rotates the incus upon its short leg, and presses its articulation with the malleus into closer contact: but the stapes is not withdrawn from under the long leg of the incus, being prevented by the strong connecting ligaments.

The smaller angle of the tendon crossing the parallel of the crura over the convex side of the basis, necessarily depresses that edge, the straight side acting as a hinge. The externus

muscle of the malleus rotates the incus back again, and restores it to its passive perpendicular situation; becoming on such occasions the antagonist of the stapedeus. It is worthy of remark, that all the muscles of the ossicula auditûs act nearly at right angles, or in straight lines, contrary to the ordinary course of muscular application, by which their forces are comparatively augmented.

The varieties in the human stapes are few: they appear in the relative curvature of the crura, and in the degree of slenderness or symmetry of its general form.

The fenestra vestibuli admits the basis of the stapes to pass into the vestibulum, when the connecting membrane is destroyed, there being no other obstacle to its descent.

None of the external similitudes in form, nor any correspondence in the habits, or voices of animals, appear to govern the configuration of these ossicles, except in those mammalia inhabiting the waters, such as the seal, walrus, and whale tribes,* where the stapes is always more massive: but in the otter, which only dives occasionally, the stapes does not vary from that of the fox. In the tiger, dog, and other feræ, the crura are straight, meeting in an acute angle; but the same figure occurs in the horse, beaver, goat, and many more herbivorous quadrupeds; so that no inference can be drawn from these different habits of life.

In the cete, exemplified in the Plate by the porpoise, whose organs of hearing precisely resemble those of whales which I have seen, and agree with the descriptions of others by Professor CAMPER, the muscle of the stapes pulls the capitulum at an angle of 45 degrees, with the plane of the basis, so as

* I have not had an opportunity of examining the ossicles of a hippopotamus.

remarkably to depress its subjacent end into the fenestra vestibuli; besides the thickness of the basis, and its exact adaptation to the fenestra, exhibit a joint of considerable motion. In those animals there is only a small perforation, instead of the crural arch. *Vide* letter *n*.*

I have discovered a very remarkable singularity, in tracing the comparison of this bone, in the marmot, and Guinea-pig. The stapes in these animals is formed with slender crura, constituting a rounded arch, through which an osseus bolt passes, so as to rivet it to its situation. This bolt I have named *pessulus*. *Vide* letter *l*. It is placed near the top of the arch, so that by the action of the stapedeus muscle the upper part of the straight crus is brought into contact with the *pessulus*; and by this means the depression of the basis is limited. It does not seem obvious for what further end this provision is designed, because, excepting the shrill whistle, there is nothing peculiarly different in the habits of those animals from others which are destitute of such mechanism.

The kanguroo has this bone like the corresponding ossicle in birds, called Columella; but it has also the malleus and incus, which birds have not.

In the ornithorhynchus paradoxus, and ornithorhynchus hystrix, the resemblance to the columella is still more striking; and forms an additional point of similarity between these strange quadrupeds and birds. Their columellæ are not, how-

* The stapes of the seal has solid rounded crura, and a small aperture; that of the walrus is entirely solid, and the edges as well as the plane of the sides, are a little twisted, agreeing with the observation of M. CUVIER, *Leçons d'Anatomie comparée*, Tome II. p. 505. In all these aquatic mammalia the fenestra rotunda, called also fenestra cochleæ, is large, being three or four diameters more than in other animals of similar bulk.

ever, articulated to a cartilage, as in birds; but to a small bone performing the office of the manubrium of the malleus.

In birds, a slender bone passes to the fenestra vestibuli, from a cartilage fixed to the membrana tympani: it is called columella, having received that name from JULIUS CASSERIUS.

The capitulum of the columella in birds is slightly expanded, and is joined to an obtuse-angled triangular plate of cartilage, which I have called cartilago columellæ, *(vide* letter *t,)* the longest side of the triangle is attached to the membrana tympani. In some species of birds a small foramen occurs in the middle of this plate, but in many others it is entire.

A strong muscle is inserted into the shorter angle of the cartilage, which draws it downward, and thus elevates the opposite angle in the center of the membrana tympani, so as to render it conical externally. Two lateral ligaments steady the articulation of the cartilage with the head of the columella.

The columellæ in birds are less brittle than the ossicula auditûs in the mammalia; their bases are exactly fitted to the fenestra vestibuli; and that part of those columellæ nearest the base is generally of a reticulated texture.

The amphibia are provided with columellæ, in their form and adaptations resembling those of birds: the cartilage is here, however, united to the under surface of the true skin, without any apparent application of muscles to alter its tension. The substance of the columella is even less hard than in birds; and its basis is considerably smaller than the fenestra vestibuli. The cavity of the tympanum has no lateral cells, and the Eustachian tube is short, and wide, seemingly for the purpose of receiving sounds conveyed through the medium of air.

From the evidence of these facts, together with the com-

parative view exhibited in the Plate, I am led to the following conclusions. In man, and the most numerous orders of the mammalia, the figure of the stapes is an accommodation to that degree of lightness which, throughout the series of ossicles, seems a requisite condition. It is also a conductor of vibrations in common with the other ossicles: but most especially it is designed to press on the fluid contained in the labyrinth by that action which it receives from the stapedeus muscle, and the hinge-like connection of the straight side of its basis with the fenestra vestibuli; the ultimate effect of which is an increase of the tension of the membrane closing the fenestra cochleæ.

It does not appear that any degree of motion ever subsists between the ossicula auditûs as wholes, which bears any relation to the peculiar vibrations of sounds; but rather that the different motions of these bones only affect the membrana tympani, and alter the degrees of contact in their articulations, so as to influence the intensity of violent impulses; sounds of less impetus, not requiring such modulation, are transmitted through the conducting series by the vibrations of the integrant parts of these bones, unaccompanied by muscular action.

This reasoning is suggested by the columellæ in the aves and amphibia: and as many birds are known to imitate a variety of artificial sounds with great accuracy, it may be inferred that they hear such sounds as acutely, and with the same distinctness as mankind.

It seems that all the muscles of the ossicula auditûs are of the involuntary kind, and the peculiar stimulus to their action is sound. The chorda tympani, which supplies them, is a ganglíated nerve: if this supposition be true, then the muscles should be considered as all acting together, and it is well

known that persons who hear imperfectly are more sensible to sounds in a noisy place, as if the muscles were by that means awakened to action.

The office which the basis of the stapes holds, and which the stapedeus muscle is especially destined to perform, seems to throw considerable light on the use of the cochlea. It cannot be allowed that the pressure of the watery fluid in the labyrinth is a requisite condition to produce the sensation of hearing, since all birds hear without any mechanism for that purpose, but as such pressure must ultimately give increased tension to the fenestra cochleæ, it follows that we inquire at this part for the principal use of the stapes.

As the membrane of the fenestra cochleæ is exposed to the air contained within the cavity of the tympanum, it appears adapted to receive such sounds as pass through the membrana tympani, without exciting consonant motions in the series of ossicula auditûs.

Experiment.

My head being laid on a table, with the meatus auditorius externus perpendicular to the horizon, my friend, Mr. WILLIAM NICHOLSON, pulled the tragus toward the cheek, and dropped from a small vial, water, at the temperature of my body, into the meatus. The first drop produced a sensation like the report of distant cannon, and the same effect succeeded each following drop, until the cavity was filled.

In this experiment the vibrations of the membrana tympani must have been impaired, if not wholly destroyed, by the contact and pressure of the water; yet the motions of the whole membrane, from the blow of each drop of water, affected

the air contained in the tympanum sufficiently to produce a sensible impression.

That something like this occurs in many kinds of sounds is more than probable; and as the cochlea consists of two hollow half cones, winding spirally, and uniting at their apices, it follows that the sounds affecting either the cone terminating in the vestibulum, or that which forms the fenestra cochleæ, must each pass from the wide to the narrow end; and the tension of the parts, in either case, will necessarily aid the impression.

I have already trespassed beyond the usual limits, and must reserve the more ample details of this subject for a work expressly directed to the anatomy and physiology of the organs of hearing.

EXPLANATION OF PLATE IV.

a, The left* stapes of a human ear magnified two diameters; presenting the curved edge of the basis, and the more elevated and pointed arch.

b, The opposite side of the same stapes, shewing its rounded arch.

c, Two figures, the uppermost being the articulating surface of the capitulum, and the one beneath shewing the under surface of the basis, of the same stapes.

d, Stapes of a hedge-hog, (*Erinaceus Europæus*,) magnified four diameters.

e, Stapes of a mole, (*Talpa Europæa*,) magnified six times.

f, Stapes of the musk ox, (*Bos moschatus*,) twice magnified.

* The other stapedes are all from the right ears.

g, Stapes of the elephant, (*Elephas maximus*,) natural size.

h, Stapes of the tiger, (*Felis Tigris*,) twice magnified.

i, Stapes of the dog, (*Canis familiaris*,) three times magnified.

j, Stapes of the horse, (*Equus Caballus*,) twice magnified.

k, Stapes of the pig, (*Sus Scrofa*,) three times magnified.

l, Stapes of the marmot (*Arctomys Marmota*) with its pessulus, magnified four times.

m, Stapes of the seal, (*Phoca vitulina*,) twice magnified.

n, Stapes of the porpoise, (*Delphinus Phocæna*,) twice magnified.

o, Stapes of the walrus, (*Trichechus rosmarus*,) natural size.

p, Stapes of the kanguroo, (*Macropus Kanguroo*,) four times magnified.

q, View of the under surface of its basis.

s, Columella of the duck-bill, (*Ornithorhynchus paradoxus*,) magnified four times.

r, Basis of the same columella.

t, Columella and cartilago columellæ of a goose, (*Anas Anser*,) twice magnified.

u, Columella of the Egyptian ibis, (*Tantalus Ibis*,) taken from a mummy, three times magnified.

v, Columella of a turtle, (*Testudo Midas*,) natural size, with its cartilage.

w, Columella of the Gangetic crocodile, (*Lacerta Gangetica*,) natural size.

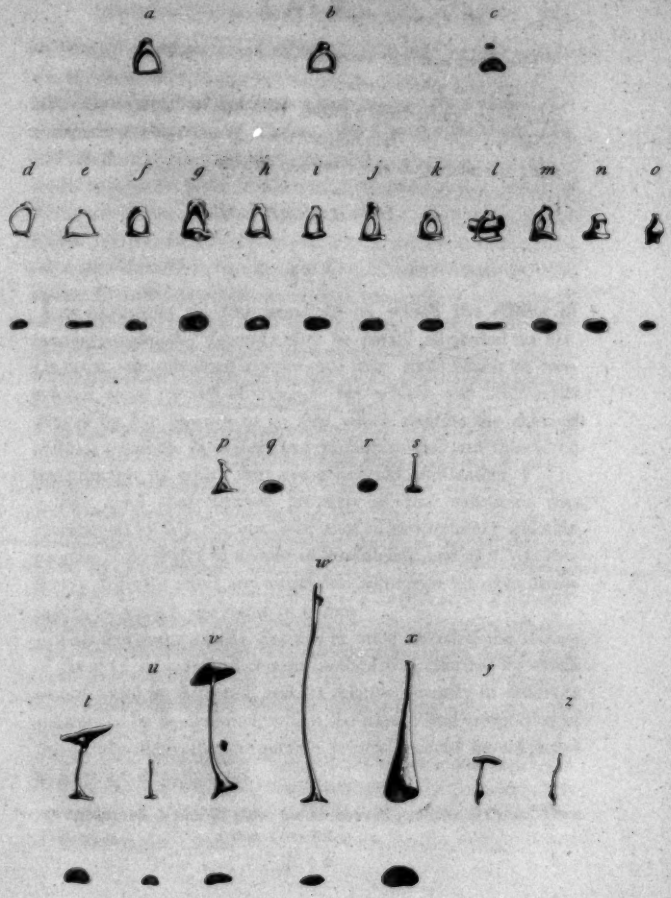
x, Columella of a turtle, (*Testudo coriacea*,) natural size.

y, Columella and cartilage of a frog, (*Rana temporaria*,) twice magnified.

z, Columella of a toad, (*Rana Bufo*,) twice magnified.

The third and last lines of objects in the Plate, exhibit the outlines, and under surfaces, of the bases of the stapedes, and columellæ, immediately above. In some, the surface is convex, in others concave, but neither the one nor the other are constant attendants on any common affinity.

STAPEDES and COLUMELLÆ compared.



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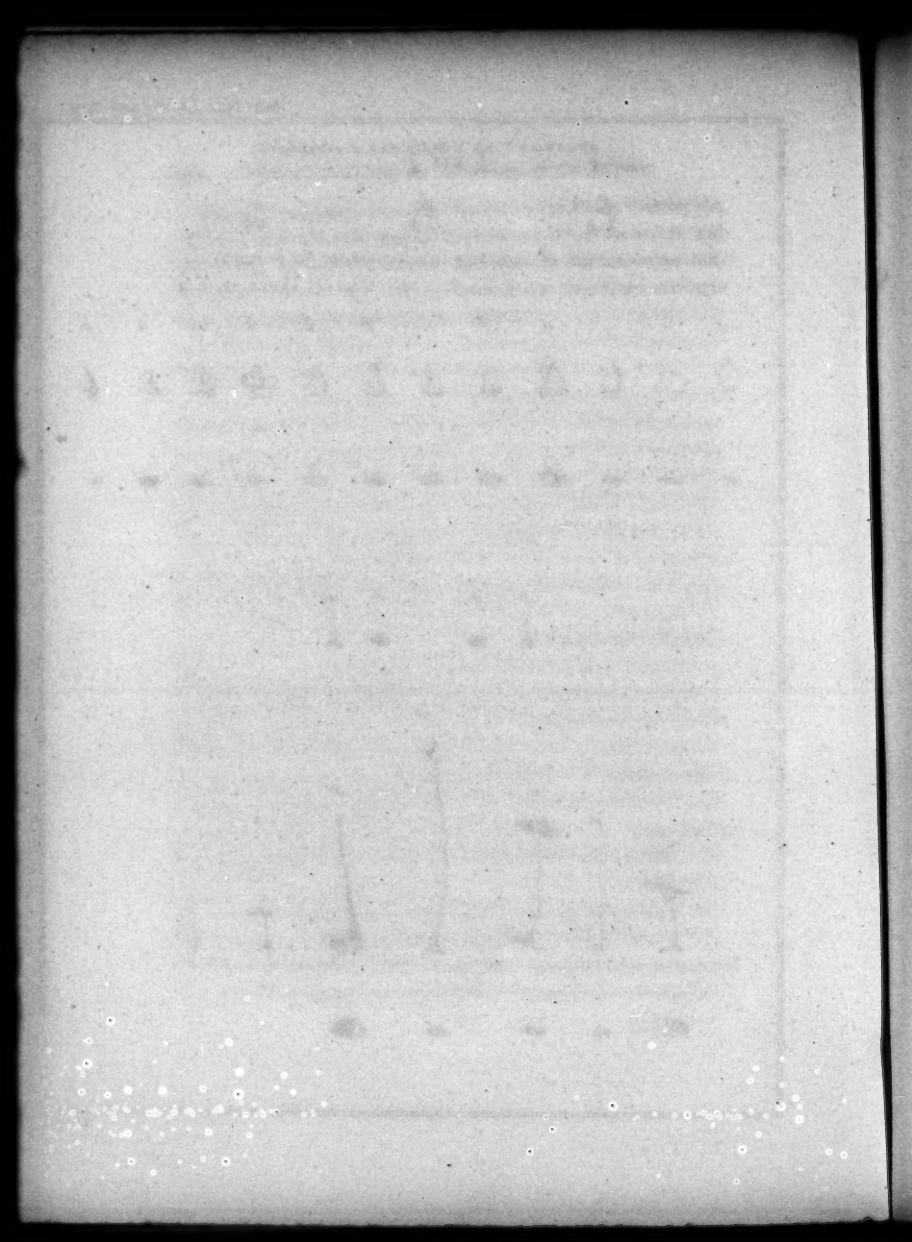

XII. *On an artificial Substance which possesses the principal characteristic Properties of Tannin.* By Charles Hatchett, *Esq. F. R. S.*

Read April 25, 1805.

§ 1.

THE discovery of the principle on which the effects of tanning essentially depend, may be partly attributed to Mr. DEYEUX, who obtained a substance from galls which he considered as a species of resin,* but which was afterwards proved by Mr. SEGUIN to be that which renders the skins of animals insoluble in water, and imputrescible, and thus to be the principle by which they are converted into leather.†

The chief characteristic property of this substance was ascertained by Mr. SEGUIN to be that of precipitating gelatine or glue from water in a state of insolubility, and as it was evidently different from any vegetable substance hitherto discovered, he gave it the name of tannin.

This discovery of Mr. SEGUIN at once unveiled the theory of the art; an easy and certain method was afforded by which tannin could be detected, and its relative quantity in different substances be determined, whilst the nature and properties of this newly discovered vegetable principle could be subjected to accurate investigation.

* *Mémoire sur la Noix de Galle*, par M. DEYEUX; *Annales de Chimie*, Tome XVII. p. 23. † Ibid. Tome XX. p. 15.

The former has derived elucidation from the experiments of Mr. BIGGIN,* and much has been contributed in every respect by Mr. PROUST,† but the subject has received the greatest extension and some of the most valuable additions from the ingenious labours of Mr. DAVY, particularly the discovery of the important fact, that catechu or terra japonica consists principally of tannin.‡

The united results of the experiments made by these and other eminent chemists, appear therefore to have fully established, that tannin is a peculiar substance or principle which is naturally formed, and exists in a great number of vegetable bodies, such as oak-bark, galls, sumach, catechu, &c. &c. commonly accompanied by extract, gallic acid, and mucilage.

But no one has hitherto supposed that it could be produced by art, unless a fact noticed by Mr. CHENEVIX may be considered as some indication of it.

Mr. CHENEVIX observed, that a decoction of coffee-berries did not precipitate gelatine unless they had been previously roasted; so that tannin had in this case either been formed or had been developed from the other vegetable principles by the effects of heat.§

Some recent experiments have however convinced me, that a substance possessing the chief characteristic properties of tannin may be formed by very simple means, not only from vegetable, but even from mineral and animal substances.

* Phil. Trans. 1799, p. 259.

† *Annales de Chimie*, Tome XXV. p. 225. Ibid. Tome XLI. p. 331. Ibid. XLII. p. 89. ‡ Phil. Trans. 1803, p. 233.

§ Nicholson's Journal for 1802, Vol. II. p. 114.

§ II.

In the course of my experiments on lac, and on some of the resins, I had occasion to notice the powerful effects produced on them by nitric acid, and I have since observed, that by long digestion, almost every species of resin is dissolved, and is so completely changed, that water does not cause any precipitation, and that by evaporation a deep yellow viscid substance is obtained, which is equally soluble in water and in alcohol, so that the resinous characters are obliterated.

When I afterwards had discovered a natural substance, which was composed partly of a resin similar to that of recent vegetables, and partly of asphaltum,* I was induced to extend the experiments already mentioned to the bitumens, in the hope of obtaining some characteristic properties by which the probable original identity of these bodies with vegetable substances might be farther corroborated. In this respect I succeeded, in some measure better than I expected; but I observed a very material difference between the solutions of the resins and those of many of the bitumens, such, for instance, as asphaltum and jet. The first effect of nitric acid, during long digestion with these substances, was to form a very dark brown solution, whilst a deep yellow or orange coloured mass was separated, which by subsequent digestion in another portion of nitric acid was completely dissolved, and by evaporation was converted into a yellow viscid substance, equally soluble in water and in alcohol, so as to perfectly resemble that which by similar means had been obtained from the resins, excepting, that when burned, it emitted an odour somewhat resembling that of the fat oils.

* Phil. Trans. 1804, p. 385.

It therefore appeared to me, that the first or dark brown solution had been formed by the action of the nitric acid on the *uncombined* carbonaceous part of the bitumens, or that by which they are rendered black, and that the deep yellow portion which was separated, was that which constituted the real or essential part of these bituminous substances. This opinion was confirmed by some experiments which I purposely made upon amber, and having every reason therefore to believe, that the dark brown solution obtained from asphaltum and jet was in fact a solution of coal, I repeated the experiments on several varieties of the pit or mineral coal, from all which, I obtained the dark brown solution in great abundance; but those coals, which contained little or no bitumen, did not yield the deep yellow substance which has been mentioned.

In each experiment I employed 100 grains of the coal, which I digested in an open matrass with one ounce of nitric acid diluted with two ounces of water. (The specific gravity of the acid was 1.40.)

After the vessel had been placed in a sand-bath, and as soon as it became warm, a considerable effervescence attended with much nitrous gas was produced; after about two days I commonly added a second and sometimes a third ounce of the acid, and continued the digestion during five or six days, or until the whole, or nearly the whole, was dissolved, excepting in those cases when the deep yellow substance was formed; for this I constantly separated.

The next experiment was made upon charcoal, which was more readily dissolved than the preceding substances, without leaving any residuum; the solution was perfect, and the colour was reddish-brown.*

* The solubility of charcoal in nitric acid, and some of its properties when thus

Having thus by means of nitric acid obtained solutions from asphaltum, from jet, from several of the pit-coals, and from charcoal, I evaporated them to dryness in separate vessels, taking care in the latter part of the process to evaporate very gradually, so as completely to expel the remainder of the acid without burning the residuum; this, in every case, proved to be a brown glossy substance, which exhibited a resinous fracture.

The chemical properties of these residua were as follows.

1. They were speedily dissolved by cold water and by alcohol.

2. Their flavour was highly astringent.

3. Exposed to heat, they smoked but little, swelled much, and afforded a bulky coal.

4. Their solutions in water reddened litmus-paper.

5. The same solutions copiously precipitated the metallic salts, especially muriate of tin, acetite of lead, and oxysulphate of iron. The colour of these precipitates was commonly brown, inclining to that of chocolate, excepting the tin, which was blackish-gray.

6. They precipitated gold from its solution, in the metallic state.

7. They also precipitated the earthy salts, such as the nitrates of lime, barytes, &c. &c.

8. The fixed alkalis, as well as ammonia, when first added

dissolved, have been noticed by Professor Lichtenstein in Crell's Chemical Annals, 1786; by Mr. Lowitz; (Crell's Chem. Journal, translated into English, Vol. II. p. 255;) and by Mr. Jameson, in his Outline of the Mineralogy of the Shetland Islands, &c. 8vo. edit. p. 167.

to these solutions only deepened the colour, but, after some hours, rendered them turbid.

9. Glue or isinglass was immediately precipitated by these solutions from water, and the precipitates were more or less brown according to the strength of the solutions. The precipitates were also insoluble in cold and in boiling water, so that in their essential properties they proved similar to those formed by the varieties of tannin hitherto known, with the difference, that this factitious substance appeared to be exempt from gallic acid, and mucilage, which commonly accompany the varieties of tannin, and which occasion modifications in the colour and appearance of some of their precipitates.

Having thus had the satisfaction to discover that a product resembling tannin could be formed by such a simple method, not only from vegetable but also from mineral coal, I was induced to examine how far the same might be extended to animal coal, and I therefore reduced a portion of isinglass to that state in a close vessel, and having rubbed it into fine powder, I digested it with nitric acid in the manner which has been described. At first the acid did not appear to act upon it, but at length it was slowly dissolved excepting a small quantity, which however was in every respect unchanged; and here we may remark, that as animal coal is incinerated with much more difficulty than vegetable coal or charcoal, so was the same difference to be observed, when oxygen was presented to these bodies in the humid way.

The solution resembled those which have been described, excepting, that the brown colour was more intense. It was evaporated to dryness, and was then dissolved in distilled

water, after which, the solution being examined by the re-agents which had been employed in the former experiments, was found to produce similar effects, excepting some difference in the colour of the precipitates.

I next added some of the liquid to a solution of isinglass, and obtained a copious precipitate. This is evident, that a tanning substance may be formed from animal as well as from vegetable and mineral coal; and it is not a little curious, that this enables us to assert as a matter of fact, although not of economy, that one portion of the skin of an animal may be employed to convert the other into leather.

In the course of these experiments, I also subjected coak to the action of nitric acid, and obtained a product which resembled that which had been afforded by pit-coal; but in this case (as might be expected) there was not any appearance of the deep yellow substance which has so often been mentioned.

These experiments therefore prove, that a tanning substance may be artificially formed by exposing carbon to the action of nitric acid; and it also appears, that this is best effected when the carbon is uncombined with any other substance excepting oxygen. The following experiments seem to corroborate this opinion.

1. A piece of Bovey coal, which had perfectly the appearance of half-charred wood, was reduced to powder, and was digested with nitric acid until the whole was dissolved.

The colour of the solution was deep yellow; and, by evaporation, a yellow viscid mass was obtained, which was dissolved in distilled water. This solution was then examined by various re-agents, and particularly by gelatine, but not any

vestige of tanning matter could be discovered, and the predominant substance appeared to be oxalic acid.

2. Another piece of Bovey coal, which had less of the characters of wood, and was more perfectly carbonized, was treated in the way which has been described; the solution was brown, and, unlike the former, afforded a considerable precipitate with gelatine.

3. A portion of the first sort of Bovey coal was exposed to a red heat in a close vessel, and was then reduced to powder and digested with nitric acid; here a remarkable difference was to be observed, for nearly the whole was thus converted into the tanning substance.

4. A coal from Sussex, extremely like the second sort of Bovey coal, also afforded the same product.

5. A piece of the Surturbrand from Iceland yielded a similar result.

6. Some deal saw-dust was digested with the nitric acid until it was completely dissolved; by evaporation a yellow viscid mass was obtained, the solution of which in water afforded results like those of the first experiment on the Bovey coal, for oxalic acid was found in it, but not any of the tanning substance.

7. Another portion of the same deal saw-dust was converted into charcoal in a close vessel; the charcoal was then treated in the manner already described, and was thereby formed into a liquid which copiously precipitated gelatine.

8. Having previously ascertained that teak wood does not contain gallic acid nor tannin, I reduced some of it into charcoal, which was afterwards readily converted into the substance above mentioned.

In these experiments, the deal and the teak wood had been reduced to the state of coal, as usual, by fire; but as this does not appear to have been the means generally employed by nature to convert organized substances into the varieties of mineral coal, I for a considerable time, previous to the discovery of the artificial tanning product, had been employed in a series of experiments on the slow carbonization of a great number of vegetable substances by the humid way.

The agent which I most commonly used to produce this effect, was sulphuric acid occasionally diluted; and although many of the processes were extremely unpleasant and tedious, yet I have not any reason to regret the time which has been thus employed. The subject however I foresee will branch out into several details, none of which as yet I can regard as sufficiently completed to merit the honour of being submitted to this learned Society; but I am in a manner almost compelled in the present case to anticipate a few of the experiments, with their results, because they are intimately connected with the subject now under consideration.

In these experiments, I have observed that concentrated sulphuric acid, when poured upon any of the resinous substances reduced to powder, dissolved them in a few minutes; at this period the solution was transparent, commonly of a yellowish-brown colour, and of the consistency of a viscid oil. But if, after this, the vessel was placed on a sand-bath, the colour of the solution became progressively darker, sulphureous gas was evolved, and at length the whole appeared like a very thick liquid of an intense black. I purposely for the present pass over many phenomena, some of which are peculiar to the different substances when thus treated, whilst others are

general, and may be referred to those attendant on etherification, for my intention here is only to notice, in a concise manner, such as immediately tend to elucidate the subject of this Paper.

When concentrated sulphuric acid is poured on the common turpentine of the shops, it almost immediately dissolves it like the solid resins; and if a portion of this solution be poured into cold water, the turpentine is precipitated in the solid brittle state of common yellow resin. But if a second portion of the same solution, after the lapse of an hour or more, be in like manner poured into cold water, the resin thus formed is not yellow but dark brown; and if four or five hours are suffered to elapse before a third portion is poured into water, the resin is found to be completely black. After this, supposing the digestion to be carried on during several days, or until there is no longer any production of sulphureous gas, the turpentine or resin will be found converted into a black porous coal, which, if the operation has been properly conducted, does not contain any resin, although a substance may frequently be separated by digestion in alcohol, which I shall soon have occasion to notice.

When common resin was thus treated, I obtained about 48 *per cent.* of the coal, which, after being exposed to a red heat in a loosely covered platina crucible, still amounted to 30 *per cent.* and by the slowness of its combustion and other circumstances which need not here be related, approached very nearly to the characters of some of the mineral coals.*

* The difference of the quantity of carbon, which may be obtained in the state of coal from resinous substances by the humid and by the dry way, is very considerable; we may take common resin as an example, for when two grains were exposed to simple

The effects produced by sulphuric acid upon turpentine and resin are manifestly caused by the union of the two constituent principles of the latter (namely, hydrogen and carbon) with part of the oxygen of the former, so that sulphureous acid, water, and coal are produced. I therefore availed myself of this process, by which coal could be progressively formed whilst the original substance was gradually decomposed, to make the following experiment.

A quantity of common turpentine was treated with sulphuric acid in the way which has been described, and different portions of the solution being poured at different periods into water whilst the remainder was digested during several days, I thus obtained from the same original substance, yellow resin, brown resin, black resin, and coal. I then digested a portion of each of these, as well as some of the turpentine, in separate vessels with nitric acid until they were completely dissolved, and afterwards reduced them to dryness. The different residua varied in colour from yellow to dark brown, corresponding to the substances which had been employed. These were then dissolved in distilled water, and were examined by solution of isinglass and other reagents.

1. The solution of the residuum of turpentine was pale straw colour, and did not precipitate gelatine.

2. That of yellow resin resembled the former in every respect.

3. That of the brown resin was of a deeper yellow, but in other particulars resembled the above.

4. That of the black resin on the contrary yielded a considerable portion of the tanning substance,—and

distillation in a small glass retort placed over an open charcoal fire, the residuum of coal only amounted to $\frac{1}{4}$ of a grain.

5. That of the coal afforded it in great abundance.

Hence it appears, that these different modifications of turpentine yielded the tanning substance only in proportion to the quantity of their original carbon, which, by oxidizement, had been progressively converted into coal.*

Other substances, when reduced into coal in the humid way, were in like manner formed into the tanning substance by nitric acid. In fact I found this to be the constant result, and amongst the many substances which were examined, I shall mention various kinds of wood, copal, amber, and wax, all of which, when reduced to coal by sulphuric acid, yielded similar products, by subsequent treatment with nitric acid.

But this substance may likewise be artificially produced without the help of nitric acid, although in a less proportion, as well as with some slight variations in its characteristic properties; for, as I have already observed, when any of the resins or gum resins (such as common resin, elemi, asa fœtida, &c.) have been long digested with sulphuric acid so as to assume the appearance and general characters of coal, if afterwards they are digested with alcohol, a portion is dissolved, and a dark brown solution is formed which by evaporation yields a mass soluble in water as well as in alcohol, and which precipitates gelatine, acetite of lead, and muriate of tin, but produces only a very slight effect on oxysulphate of iron. This substance, therefore, which may thus be separated by alcohol from the coal formed from resinous bodies by sulphuric acid, evidently contains some of the tanning matter, which has been produced during the carbonization of those substances.

* Some late experiments have however convinced me that carbon need not be absolutely converted into coal in order to produce the artificial tanning substance; but this will be more fully explained in a subsequent Paper.

A natural process very similar to this, I much suspect takes place in some cases where peat is formed; I say in some cases, because the production of tanning matter does not seem to be absolutely a necessary consequence attendant on the formation of peat; for in many places where the latter abounds, the former cannot be detected, whilst in others, it is very abundant, and acts powerfully on animal bodies which have accidentally been exposed to its effects.

There are many facts of this kind upon record, such as the account of the bodies of the man and woman preserved in the moors near the woodlands in Derbyshire, and also of the woman found in the morass at Axholm, in Lincolnshire.* Now I am much inclined to believe, that the tanning substance which so much abounds in these and some other peat moors, did not originally exist in the vegetable substances from which the peat has been produced, but that it has been and continues to be progressively formed (under certain favourable circumstances) during the gradual carbonization and conversion of the vegetable matter into peat.

§ III.

In most of the former papers which I have had the honour to lay before the Royal Society, I have for greater perspicuity generally concluded with a recapitulation of the contents, but in the present case, this appears to be superfluous, as the whole may be concentrated into one simple fact, namely, that a substance very analogous to tannin, which has hitherto been considered as one of the proximate principles of vegetables, may at any time be produced, by exposing carbonaceous substances,

* Phil. Trans. Vol. XXXVIII. p. 413. Ibid. Vol. XLIV. p. 571.

whether vegetable, animal, or mineral, to the action of nitric acid.

Since the preceding experiments were made, I have farther proved the efficacy of this substance by actual practice, and have converted skin into leather by means of materials which, to professional men, must appear extraordinary, such as, deal saw-dust, asphaltum, common turpentine, pit-coal, wax candle, and a piece of the same sort of skin.

Allowing, therefore, that the production of this substance must for the present be principally regarded only as a curious chemical fact not altogether unimportant, yet as the principle on which it is founded appears to be developed, we may hope, that a more economical process will be discovered, so that every tanner may be enabled to prepare his leather even from the refuse of his present materials.

The organized bodies and their products have only of late years much attracted the attention of chemists, many of whom, even at this time, (although the modes of chemical examination have been so much improved,) seem disgusted and deterred by the Proteus-like changes which take place whenever these substances are subjected to experiment.

But these variable and endless alterations of their properties seem rather calculated to operate as incitements to investigation; for by the accumulation of facts resulting from the changes produced in these bodies by disuniting and by recombining their elementary principles, not only will chemistry as a science become farther illumined and extended, but it will, as it has hitherto done, render great and essential services to the arts and manufactures.

XIII. *The Case of a full-grown Woman in whom the Ovaria were deficient.* By Mr. Charles Pears, F. L. S. Communicated by the Right Hon. Sir Joseph Banks, K. B. P. R. S.

Read May 9, 1805.

THE following case is laid before this learned Society, as an addition to those already registered in the Philosophical Transactions, with the view of elucidating such physiological inquiries as are connected with the state of the organs of generation.

ANN JOSEPH was born at Disarth, in Radnorshire, North Wales, August 4, 1770. She was of a fair florid complexion, and blue eyes, dark-brown hair, a flat nose, and thick lips. She was naturally mild, but when irritated, was sometimes malicious and revengeful. In her diet she was remarkably abstemious, eating little of animal food, no fresh vegetables, and so small a portion of bread, that she often did not consume a penny loaf in the course of a week. If at any time she was prevailed upon to take several kinds of food, her stomach was so much affected by it, that she fainted away; and if she had eaten a hearty meal, these faintings would be repeated.

She was of a costive habit, seldom having a passage in her bowels oftener than once in nine days, and sometimes only once in fourteen. She slept well, and could endure hard work, but was slow in performing it. Having ceased to grow at ten years of age, she was in stature not more than 4 feet 6 inches

high. Her breadth across the shoulders was as much as 14 inches, but her *pelvis*, (contrary to what is usually observed in the proportions of the female skeleton,) measured only 9 inches, from the *ossa ilia* across the sacrum. Her breasts and nipples never enlarged more than in the male subject; she never menstruated: there was no appearance of hair on the pubes, nor were there any indications of puberty either in mind or body, even at twenty-nine years of age; on the contrary, she always expressed aversion to young men who were too familiar with her.

At the age of twenty-one she expressed much uneasiness at finding herself different from other young women, which she attributed to not having menstruated; and was so desirous of relief that she frequently took medical advice for that purpose.

From her infancy also she was liable to complaints in her chest, attended with cough, that came upon her at intervals in violent attacks, and increased in violence as she advanced in life. In her twenty-ninth year one of these attacks came on, attended with convulsions, of which she died after a few hours illness.

After death, the female organs were taken out and preserved. In this state they were shown to Sir JOSEPH BANKS, at whose request their internal structure was accurately examined, and the following appearances were observed, which are illustrated in the annexed drawing of them, made by Mr. CLIFT.

The os tincæ and uterus had the usual form, but had never increased beyond their size in the infant state; the passage into the uterus through the cervix was oblique. The cavity of

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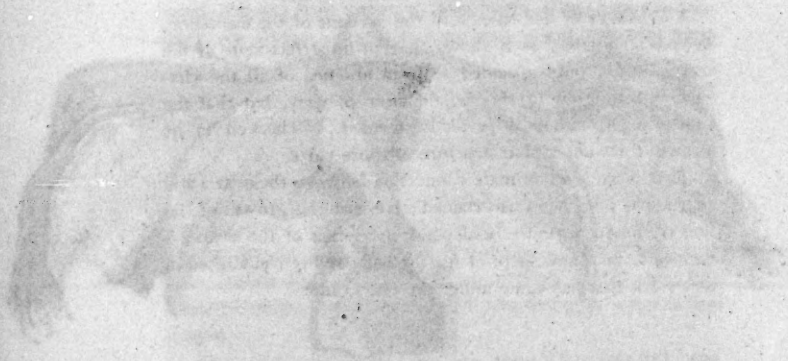


the uterus was of the common shape, and the Fallopian tubes were pervious to the fimbriæ; the coats of the uterus were membranous.

The ovaria were so indistinct as rather to show the rudiments which ought to have formed them, than any part of their natural structure. All these appearances will be better understood by their representation in the annexed drawing (Plate V.) than from any description that can be given.

The history of this case, with the account of the dissection, becomes valuable, as it shows that an imperfect state of the ovaria is not only attended with an absence of all the characters belonging to the female after puberty, but that the uterus itself, although perfectly formed, is checked in its growth for want of due structure of those parts.

That there is an intimate connection between the ovaria and uterus has long been ascertained; but that the growth of the uterus should so entirely depend upon that of the ovaria, I believe to be a new fact; at least it has not been published in any work that has come under my observation.

XIV. *A Description of Malformation in the Heart of an Infant.* *By Mr.* Hugh Chudleigh Standert. *Communicated by* Anthony Carlisle, *Esq. F. R. S.*

Read May 9, 1805.

THE child, from whom the subject of the present description was taken, died at the age of ten days; during which period all the animal functions seemed to have been regularly discharged, with this exception, that the skin exhibited the purple or blue colour, so often noticed in cases of imperfect pulmonary circulation.

The body was fleshy, somewhat less than the usual size, and the extremities were livid. All the viscera were in a natural state, except the heart, which presented the following remarkable structure.

On viewing it externally, only one auricle could be observed, into which the pulmonary veins, and venæ cavæ, entered in their ordinary directions. The pulmonary artery was wholly deficient; and, on dissection, it appeared that the body of the heart possessed but one ventricle, separated from the auricle by tendinous valves, and opening into the aorta.

The auricle was also single, having a narrow muscular band, which crossed the ostium venosum, in the place of the septum. The aorta sent off an artery, from the situation of the ductus arteriosus, which divided itself into two branches, supplying each mass of the lungs. These vessels were of small diameter.

The arterial system had been injected with wax, and, in removing the heart from the thorax, the pulmonary branch of the aorta was unfortunately cut away.

The pulmonary veins were four in number, but neither the area of these veins, nor that of the vessel which acted as the pulmonary artery, exceeded half the common dimensions.

This child, when alive, came under the observation of Dr. Combe, who did not perceive that its respiration, temperature, or muscular action, were materially affected. In the records of malformation of the heart, the present case is extraordinary, resembling in organization the amphibious animals, rather than the mammalia. That an infant should have existed so long, under such circumstances, carrying on all the vital functions, appears a physiological fact of some importance, especially as the dependence of life on respiration, and the changes produced in the vascular system, are so imperfectly understood.

EXPLANATION OF THE DRAWING. (Plate VI.)

Fig. 1. A view of the left side of the heart, the common ventricle being opened by a simple incision, showing the valves of the ostium.

a, The aorta.

b, The common trunk of the two branches of the right pulmonary veins.

c, The vena cava superior.

d, *e*, The two trunks of the left pulmonary veins.

Fig. 2. A view of the right side of the heart, exhibiting the common cavity of the auricle, a portion of its parietes being cut away, with the vena cava inferior.

a, *e*, The two trunks of the left pulmonary veins.
b, The vena cava superior.
d, The aorta.
e, The trunk of the right pulmonary veins.
f, The muscular band in the auricle.

These drawings are of the natural size, and the subject of them is preserved in the collection of Dr. Cosins.

Fig. 1.

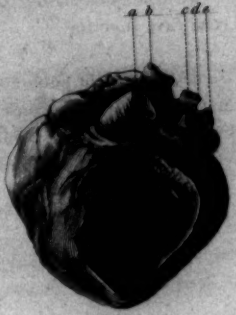
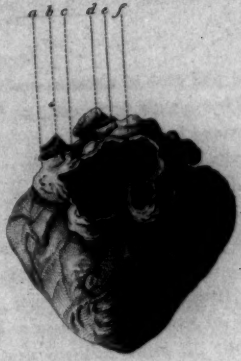


Fig. 2.



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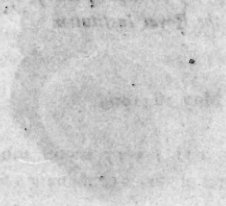


XV. *On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid.* By Humphry Davy, Esq. F. R. S. *Professor of Chemistry in the Royal Institution.*

Read May 16, 1808.

I HAVE found the boracic acid a very useful substance for bringing the constituent parts of stones containing a fixed alkali into solution. Its attraction for the different simple earths is considerable at the heat of ignition, but the compounds that it forms with them are easily decomposed by the mineral acids dissolved in water, and it is on this circumstance that the method of analysis is founded.

The processes are very simple.

100 grains of the stone to be examined in very fine powder, must be fused for about half an hour, at a strong red heat, in a crucible of platina or silver, with 200 grains of boratic acid.

An ounce and half of nitric acid, diluted with seven or eight times its quantity of water, must be digested upon the fused mass till the whole is decomposed.

The fluid must be evaporated till its quantity is reduced to an ounce and half or two ounces.

If the stone contain silex, this earth will be separated in the process of solution and evaporation; and it must be collected upon a filter, and washed with distilled water till the boracic acid and all the saline matter is separated from it.

The fluid, mixed with the water that has passed through the filter, must be evaporated, till it is reduced to a convenient quantity, such as that of half a pint, when it must be saturated with carbonate of ammonia, and boiled with an excess of this salt, till all the materials that it contains capable of being precipitated, have fallen to the bottom of the vessel.

The solution must then be separated by the filter, and the earths and metallic oxides retained.

It must be mixed with nitric acid till it tastes strongly sour, and evaporated till the boracic acid appears free.

The fluid must be passed through the filter, and subjected to evaporation till it becomes dry; when, by exposure to a heat equal to 450° FAHRENHEIT, the nitrate of ammonia will be decomposed, and the nitrate of potash or soda will remain in the vessel.

It will be unnecessary for me to describe minutely the method of obtaining the remaining earths and metallic oxides free from each other, as I have used the common processes. I have separated the alumine by solution of potash, the lime by sulphuric acid, the oxide of iron by succinate of ammonia, the manganese by hydrosulphuret of potash, and the magnesia by pure soda.

XVI. *On the Direction and Velocity of the Motion of the Sun, and Solar System.* By William Herschel, *LL. D. F. R. S.*

Read May 16, 1805.

Our attention has lately been directed again to the construction of the heavens, on which I have already delivered several detached papers. The changes which have taken place in the relative position of double stars, have ascertained motions in many of them, which are probably of the same nature with those that have hitherto been called proper motions. It is well known that many of the principal stars have been found to have changed their situation, and we have lately had a most valuable acquisition in Dr. Maskelyne's Table of proper motions of six and thirty of them. If this Table affords us a proof of the motion of the stars of the first brightness, such as are probably in our immediate neighbourhood, the changes of the position of minute double stars that I have ascertained, many of which can only be seen by the best telescopes, likewise prove that motions are equally carried on in the remotest parts of space which hitherto we have been able to penetrate.

The proper motions of the stars have long engaged the attention of astronomers, and in the year 1783, I deduced from them, with a high degree of probability, a motion of the sun and solar system towards λ Herculis. The reasons which were then pointed out for introducing a solar motion, will now be much strengthened by additional considerations; and the

above mentioned Table of well ascertained proper motions will also enable us to enter rigorously into the necessary calculations for ascertaining its direction, and discovering its velocity. When these points are established, we shall be prepared to draw some consequences from them that will account for many phenomena which otherwise cannot be explained.

The scope of this Paper, wherein it is intended to assign not only the direction, but also the velocity of the solar motion, embraces an extensive field of observation and calculation; but as to give the whole of it would exceed the compass of the present sheets, I shall reserve the velocity of the solar motion for an early future opportunity, and proceed now to a disquisition of the first part of my subject, which is the direction of the motion of the sun and solar system.

Reasons for admitting a solar Motion.

It may appear singular that, after having already long ago pointed out a solar motion, and even fixed upon a star towards which I supposed it to be directed, I should again think it necessary to show that we have many substantial reasons for admitting such a motion at all. What has induced me to enter into this inquiry is, that some of the consequences hereafter to be drawn from a solar motion when established, seem to contradict the very intention for which it is to be introduced. The chief object in view, when a solar motion was proposed to be deduced from observations of the proper motions of stars, was to take away many of these motions by investing the sun with a contrary one. But the solar motion, when its existence has been proved, will reveal so many concealed real motions, that we shall have a greater sum of them than it would be necessary

to admit, if the sun were at rest; and, to remove this objection, the necessity for admitting its motion ought to be well established.

Theoretical Considerations.

A view of the motion of the moons, or secondary planets, round their primary ones, and of these again round the sun, may suggest the idea of an additional motion of the latter round some other unknown center; and those who like to indulge in fanciful reviews of the heavens, might easily build a system upon hypotheses not altogether without some plausibility in their favour. Accordingly we find that Mr. LAMBERT, in a work which is full of the most fantastic imaginations, has framed a system wherein the sun is supposed to move about the nebula in Orion.* But, setting aside the extravagant idea of making this luminous spot a center of motion, it must certainly be admitted that the solar motion itself is at least a very possible event.

I have already mentioned, in a note to my former Paper,† that the possibility of a solar motion has also been shown from theoretical principles by the late Dr. WILSON of Glasgow; and its probability afterwards, from reasons of the same nature, by Mr. DE LA LANDE. The rotatory motion of the sun, from which he concludes a displacing of the solar center, must certainly be allowed to indicate a motion of translation in space; for though it may be possible, it does not appear probable, that any mechanical impression should have given the former, without occasioning the latter. But, as we are intirely unacquainted with the cause of the rotatory motion, the solar

* See *Système du Monde de* Mr. LAMBERT, page 152, and 158.

† See Phil. Trans. for the year 1783, page 285.

translation in space from theoretical reasons, can only be admitted as a very plausible hypothesis.

It would be worth while for those who have fixed instruments, to strengthen this argument by observing the stars which are known to change their magnitudes periodically. For as we have great reason to ascribe these regular changes to a rotatory motion of the stars,* a real motion in space may be expected to attend it; and the number of these stars is so considerable that their concurring testimony would be very desirable.

Perhaps Algol, which according to these ideas must have a very quick rotatory motion, may be found to have also a considerable progressive one; and if that should be ascertained, the position of the axis of the rotation of this star will be in a great measure thereby discovered.

An argument from the real motion to a rotatory one is nearly of equal validity, and therefore all the stars that have a motion in space may be surmised to have also a rotation on their axes.

Symptoms of parallactic Motions.

But, setting aside theoretical arguments, I shall now proceed to such as may be drawn from observation; and, as all parallactic motions are evident indications that the observer of them is not at rest, it will be necessary to explain three sorts of motions, of which the parallactic is one; they will often engage our attention in the following discussion.

Let the sun be supposed to move towards a certain part of the heavens, and since the whole solar system will have the

* See Phil. Trans. for the year 1795, page 68.

same motion, the stars must appear to an inhabitant of the earth to move in an opposite direction. In the triangle spa, Plate VII. Fig. 1, let sp represent the parallactic motion of a star; then, if this star is one that has no real motion, sp will also be its apparent motion; but if the star in the same time, that by its parallactic motion it would have gone from s to p, should have a real motion which would have carried it from s to r, then will it be seen to move along the diagonal sa, of the parallelogram $srpa$; and pa, which is parallel and equal to sr, will represent its real motion. Therefore, in the above mentioned triangle spa, which I suppose to be formed in the concave part of the heavens by three arches of great circles, the eye of the observer being in the center, the three sides will represent, or stand for, the three motions I have named. sp the parallactic, pa the real, and sa the apparent motion of the star. The situation and length of these arches, in seconds of a degree, will express, or rather represent, not only the direction but also the quantity of each motion, such as it must appear to an eye in the above mentioned central situation. And calling the solar motion S, the distance of the star from the sun d, and the sine of the star's distance from the point towards which the sun is moving ϕ, the parallactic motion, when these are given, will be had by the expression $\frac{\phi \cdot S}{r \cdot d} = sp$. This theorem, and its corollaries, of which frequent use will be made hereafter, it will not be necessary here to demonstrate.

When I call the arch pa the real motion, it should be understood that I only mean its representative; for it must be evident that the absolute motion of a star in space, as well as its intrinsic velocity, will still remain unknown, because the inclination of that motion on which also its real velocity will

depend, admits of the greatest variety of directions. We are only acquainted with the plane in which the motion must be performed, and with the length of the arch in seconds by which that motion may be measured. We may add that the chords of the arches representing the three motions are the smallest velocities of these motions that can be admitted; for in every other direction but at right angles to the line of sight, the actual space over which the star will move must be greater than the arch or chord by which its motion is represented.

Now, since a motion of the sun will occasion parallactic motions of the stars, it follows that these again must indicate a solar motion; but in order to ascertain whether parallactic motions exist, we ought to examine those stars which are most liable to be visibly affected by solar motion. This requisite points out the brightest stars as the most proper for our purpose; for any star may have a great real motion, but in order to have a great parallactic one, it must be in the neighbourhood of the sun. And as we can only judge of the distance of the stars by their splendour we ought to choose the brightest, on account of a probability that, being nearer than faint ones, they may be more within the reach of parallax, and thus better qualified to show its effects.

We are also to look out for a criterion whereby parallactic may be distinguished from real motions; and this we find in their directions. For if a solar motion exists, all parallactic motions will tend to a point in opposition to the direction of that motion; whereas real motions will be dispersed indiscriminately to all parts of space.

With these distinctions in view, we may examine the proper motions of the principal stars; for these, if the sun is not at

real, must either be intirely parallactic, or at least composed of real and parallactic motions; in the latter case they will fall under the denomination of one of the three motions we have defined, namely *ss*, the apparent motion of the star.

In consequence of this principle I have delineated the meeting of the arches arising from a calculation of the proper motions of the 36 stars in Dr. MASKELYNE's catalogue, on a celestial globe; and, as all great circles of a sphere intersect each other in two opposite points, it will be necessary to distinguish them both: for, if the sun moves to one of them, it may be called the apex of its motion, and as the stars will then have a parallactic motion to the opposite one, the appellation of a parallactic center may very properly be given to it. The latter falling into the southern hemisphere, among constellations not visible to us, I shall only mention their opposite intersections; and of these I find no less than ten that are made by stars of the first magnitude, in a very limited part of the heavens, about the constellation of Hercules. Upon all the remaining surface of the same globe there is not the least appearance of any other than a promiscuous situation of intersections; and of these only a single one is made by arches of principal stars.

The ten intersecting points made by the brightest stars are as follows. The 1st is by Sirius and Arcturus, in the mouth of the Dragon. The 2d by Sirius and Capella, near the following hand of Hercules. The 3d by Sirius and Lyra, between the hand and knee of Hercules. The 4th by Sirius and Aldebaran, in the following leg of Hercules. The 5th by Arcturus and Capella, north of the preceding wing of the Swan. The 6th by Arcturus and Aldebaran, in the neck of the Dragon. The 7th by Arcturus and Procyon, in the preceding foot of Hercules.

The 8th by Capella and Procyon, south of the following hand of Hercules. The 9th by Lyra and Procyon, preceding the following shoulder of Hercules. And the 10th is made by Aldebaran and Procyon, in the breast of Hercules.

The following Table gives the calculated situation of these ten intersections in right ascension and north polar distance.

Table I.

No.	Right Ascension.			Polar Distance.		
	°	′	″	°	′	″
1	255	39	50	36	41	34
2	275	9	32	64	21	48
3	272	23	58	58	23	24
4	268	25	38	44	39	47
5	290	0	58	32	7	23
6	267	2	19	33	57	20
7	285	8	13	46	21	34
8	272	51	49	78	7	56
9	266	46	49	66	48	11
10	260	1	29	60	59	34

We might rest satisfied with having shown that the parallactic effect of which we are in search is plainly to be perceived in the motion of the brightest stars; however, by way of further confirmation, we may take in some large stars of the next order, in whose motions evident marks of the influence of parallax may likewise be perceived. When the intersections made by their proper motions and the arches in which the stars of the first magnitude are moving, are examined, we find no less than fifteen which unite with the former ten, in pointing out the same part of the heavens as a parallactic center. It will be sufficient only to mention the opposite

points of the situation of these intersections, and the stars by which they are made, without giving a calculated table of them.

The 1st is the following leg of Hercules, and is made by Sirius and β Tauri. The 2d is also in the following leg of Hercules, by Sirius and α Andromedæ. The 3d is in the following hand of Hercules, by Sirius and α Arietis. The 4th in the neck of the Dragon, by Arcturus and β Tauri. The 5th between the Lyre and the northern wing of the Swan, by Capella and α Andromedæ. The 6th near the following hand of Hercules, by Capella and α Arietis. The 7th preceding the head of Hercules, by Lyra and β Tauri. The 8th between the Lyre and northern wing of the Swan, by Lyra and α Andromedæ. The 9th in the following arm of Hercules, by Lyra and α Arietis. The 10th in the following leg of Hercules, by Aldebaran and β Tauri. The 11th in the following leg of Hercules, by Aldebaran and α Andromedæ. The 12th in the head of Hercules, by Aldebaran and α Arietis. The 13th in the following arm of Hercules, by Procyon and β Tauri. The 14th in the back of Hercules, by Procyon and α Andromedæ. And the 15th near the following arm of Hercules, is made by Procyon and α Arietis.

An argument like this, founded upon the most authentic observations, and supported by the strictest calculations, can hardly fail of being convincing. And though only the ten principal apices of the twenty-five that are given have been calculated, the other fifteen may nevertheless be depended upon as true to less than one degree of the sphere.

Changes in the Position of double Stars.

We have lately seen that the alterations in the relative situation of a great number of double stars may be accounted for by a parallactic motion. Among the 56 stars which I have given, the changes of more than half of them appear to be of this nature; and it will certainly be more eligible to ascribe them to the effect of parallax than to admit so many separate motions in the different stars; especially when it is considered that if the alterations of the angle of position were owing to a motion of the largest star of each set, the direction of such motions must, in contradiction to all probability, tend nearly to one particular part of the heavens.

This argument, drawn from the change of the position of double stars, may be considered as deriving its validity from the same source with the former, namely, the parallactic motions of at least 28 more stars, pointing out the same apex of a solar motion by their direction to its opposite parallactic center.

Incongruity of proper Motions.

It may be remarked that the proper motions of the stars, if they were in reality such as they appear to be, would contain a certain incongruous mixture of great velocity and extreme slowness. Arcturus alone describes annually an arch of more than two seconds: Aldebaran hardly one-tenth and a quarter of a second: Rigel little more than one-tenth and a half: even Lyra moves barely three and a quarter tenths of a second, while Procyon has almost four times that velocity. Out of 36 stars whose proper motion we have examined, there are 15 that do not reach two-tenths of a second: β Virginis moves

seventy-seven hundredths, and *a* Cygni only six. But it will be shown, when the direction and velocity of the solar motion come to be explained, that these kind of incongruities are mere parallactic appearances; and that there is so general a consistency among the real motions of the stars, that Arcturus is in no respect singled out as a star whose motion is far beyond the rest.

By giving this remark a place among the reasons for admitting a solar motion, it is not intended to lay any particular stress upon it; for it may be objected that our idea of the congruence or harmony of the celestial motions can be no criterion of their real fitness and symmetry. But when such discordant proper motions as those I have mentioned in stars of no very different lustre are under consideration, and may be easily shown to be only parallactic phenomena, the method by which this can be done must certainly appear eligible, and when added to many other inducements, will throw some share of weight into the scale.

Sidereal Occultation of a small Star.

Of nearly the same importance with the former argument is the account of the occultation of a small star by a large one, which I have given in my last Paper. When the solar motion has been established, we shall prove that the vanishing of the small star near *β* Cygni, as far as we can judge at present, is only a parallactic disappearance. It must be granted that a real motion of the large star would also explain the same phenomenon; but then again, this star must be supposed to move towards the very same parallactic center which the changes in the

position of other double stars point out, and this cannot be probable.

Direction of the solar Motion.

From what has been said, I believe the expedience of admitting a solar motion will not be called in question: our next endeavour therefore must be to investigate its direction.

To return to the before mentioned intersections of the arches, in which the proper motions of the stars are performed, I shall begin by proving that when the proper motions of two stars are given, an apex may be found, to which, if the sun be supposed to move with a certain velocity, the two given motions may then be resolved into apparent changes, arising from sidereal parallax, the stars remaining perfectly at rest.

Let the stars be Arcturus and Sirius, and their annual proper motions as given in the Astronomer Royal's Tables.

When the annual proper motion of Arcturus, which is $-1'',26$ in right ascension, and $+1'',72$ in north polar distance, is reduced by a composition of motions to a single one, it will be in a direction which makes an angle of $55^\circ\ 29'\ 42''$ south-preceding with the parallel of Arcturus, and of a velocity so as to describe annually $2'',08718$ of a great circle.

The annual proper motion of Sirius, $-0'',49$ in right ascension, and $+1'',04$ in north polar distance, by the same method of composition, becomes a motion of $1'',11528$, in a direction which makes an angle of $68^\circ\ 49' 41''$ south-preceding with the parallel of Sirius.

By calculation, the arches in which these two stars move, when continued, will meet in what I have called their parallactic center, whose right ascension is $75^\circ\ 39'\ 50''$, and south polar

distance is 36° 41′ 34″. The opposite of this, or right ascension 253° 39′ 50″, and north polar distance 36° 41′ 34″, is what we are to assume for the required apex of the solar motion.

When a star is situated at a certain distance from the sun, which we shall call 1; and 90° from the apex of the solar motion, its parallactic motion will be a maximum. Let us now suppose the velocity of the sun to be such that its motion, to a person situated on this star, would appear to describe annually an arch of 2″,84825, or, which is the same thing, that the star would appear to us, from the effect of parallax, to move over the above mentioned arch in the same time.

To apply this to Arcturus, we find by calculation that its distance from the apex of the solar motion is 47° 7′ 6″; its parallactic motion therefore, which is as the sine of that distance, will be 2″,08718; and this, as has been shown, is the apparent motion which observation has established as the proper motion of Arcturus.

In the next place, if we admit Sirius to be a very large star situated at the distance 1,6809 from us, and compute its elongation from the apex of the solar motion, we shall find it 138° 50′ 14″,5. With these two data we calculate that its parallactic motion will be $\frac{\rho \cdot S}{r \cdot d} = sp = 1''{,}11528$; and this also agrees with the apparent motion which has been ascertained by observation as the proper motion of Sirius.

Now since, according to the rules of philosophising, we ought not to admit more motions than will account for the observed changes in the situation of the stars, it would be wrong to have recourse to the motions of Arcturus and Sirius, when that of the sun alone will account for them both; and this consideration would be a sufficient inducement for us to

fix at once on the calculated apex, as well as on the relative distances that have been assigned to these stars, if other proper motions could with equal facility be resolved into similar parallactic appearances. But from the nature of proper motions, it follows, that when a third star does not lead us to the same apex as the other two, its apparent motion cannot be resolved by the effect of parallax alone. And to enhance our difficulties, the number of apices, that would be required to solve all proper motions into parallactic ones, increases not as the number of stars admitted to have proper motions, but, when their situation happens to be favourable, as the sum of an arithmetical series of natural numbers, beginning at 0, continued to as many terms as there are stars admitted: so that if two stars give only one apex, one star added to it will give three apices; and ten, for instance, will give no less than 45, and so on.

The method of reasoning which, on this subject, I have adopted, is so closely connected with astronomical observations that I shall keep them constantly in view; and therefore shall illustrate what has been advanced, by taking in Capella as a third star. The three apices which then are pointed out will be that in the mouth of the Dragon, by Arcturus and Sirius; a second under the northern wing of Cygnus, by Arcturus and Capella; and a third in the following hand of Hercules, by Sirius and Capella. The calculation of them is in Table I.

The annual proper motions of our third star in Dr. MASKELYNE's Tables are + 0″.21 in right ascension, and + 0″.44 in north polar distance; and by calculation these quantities give an annual motion of 0″.46874 to Capella, in a direction which makes an angle of 71° 35′ 22″.4 south-following with the parallel of this star.

The distance of Capella from the same calculated apex of the solar motion, by which we have already explained the apparent motions of the other two stars, is 80° 54′ 40″; and, admitting again the velocity of the sun towards the same point as stated before, it will occasion a parallactic motion of Capella, in a direction 89° 54′ 48″ south-following its parallel, amounting to 2″,8125. In this calculation Capella has been taken for a star of the first magnitude, supposing its distance from us to be equal to that of Arcturus.

By constructing then a triangle, the three sides of which will represent the three motions which every star must have that is not at rest in space; we have one of the sides, representing the apparent motion of the star, equal to 0″,4687; the other side, being the parallactic motion of the star 2″,8125; and the included angle 18° 19′ 27″. From these data we obtain the third side, representing the real motion of the star, which will be 2″,3757. By the given situation of this triangle with respect to the parallel of declination of Capella, the angle of the real motion will also be had, which is 86° 34′ 11″ north-following the parallel of this star. A composition of the parallactic and the real motion in the directions we have assigned, will produce the annual apparent motion which has been established by observation.

But to apply what has been said to our present purpose, it may be observed, that although we have accounted for the proper motion of our third star by retaining the same apex of the solar motion, which has given us an explanation of the apparent motions of the other two, yet in doing this we have been obliged to assign a great degree of real motion to Capella: and to this it may be objected, that we can have no authority

to deprive Arcturus and Sirius of real motions, in order to give one of the same nature to our third star: and indeed to every star that has a proper motion which does not tend to the same parallactic center as the motions of Arcturus and Sirius.

This objection is perfectly well founded, and I have given the above calculation on purpose to show that, when we are in search of an apex for the solar motion, it ought to be so fixed upon as to be equally favourable to every star which is proper for directing our choice. Hence a problem will arise, in our present case, how to find a point whose situation among three given apices shall be so that, if the sun's motion be directed towards it, there may be taken away the greatest quantity of proper motion possible from the given three stars. The intricacy of the problem is greater than at first it may appear, because by a change of the distance of the apex from any one of the stars, its parallactic motion, which is as the sine of that distance, will be affected; so that it is not the mere alteration of the angle of direction, which is concerned. However, it will not be necessary to enter into a solution of the problem; for it must be very evident that a much more complex one would immediately succeed it, since three stars would certainly not be sufficient to direct us in our present endeavour to find the best situation of an apex for the solar motion; I shall therefore now leave these stars, and the apices pointed out by them, in order to proceed to a more general view of the subject.

We have already seen that the brightest stars are most proper for showing the effect of parallax, and that in our search after the direction of the solar motion, our aim must be to reduce the proper motions of the stars to their lowest

quantities. The six principal stars, whose intersecting arches have been given, when their proper motions in right ascension and polar distance are brought into one direction, will have the apparent motions contained in the following Table.

Table II.

Names of the Stars.	Direction of the apparent Motions.	Quantities of the apparent Motions.
Sirius -	68° 49′ 40″,7 south-preceding	1″,11528 per year
Arcturus -	35 29 42,0 south-preceding	2,08718 ———
Capella -	71 35 22,4 south-following	0,46374 ———
Lyra - -	56 20 57,3 north-following	0,32485 ———
Aldebaran	76 29 37,3 south-following	0,12341 ———
Procyon -	30 2 24,5 south-preceding	1,28941 ———
	Sum of the apparent motions	5″,35837

We must now recur to what has been said, when the construction of the triangle expressing the three motions of a star, that is not at rest, was explained; and, as we are to find out a solar motion which will require the least real motion in our six stars, an attention to this triangle will be of considerable use; for when the line *pa*, Fig. 1, which represents the real motion, is brought into the situation *ma*, where it is perpendicular to *sp*, the real motion which is required will then be a minimum. It also follows, from the construction of the same triangle, that if by the choice of an apex for the solar motion we can lessen the angle made at *s* by the lines *sp* and *sa*, we shall lessen the quantity of real motion required to bring the star from the parallactic line *spm* to the observed position *a*.

It has already been shown, in the case of Sirius and Arcturus,

that when two stars only are given, the line sp may be made to coincide with the lines sa, of both the stars, whereby their real motions will be reduced to nothing. It has also been proved, by adding Capella to the former two, that when three stars are concerned, some real motion must be admitted in one of them. Now, since all parallactic motions are directed to the same center, a single line may represent the direction of the effect of the parallax, not only of these three stars but of every star in the heavens. According to this theory, let the line sP or sS, in Fig. 2, stand for the direction of the parallactic motion of the stars; and as in the foregoing Table we have the angles of the apparent motion of six stars with the parallel of each star, we must now also compute the direction of the line sP or sS with the parallels of the same stars. This may be done as soon as an apex for the solar motion is fixed upon. The difference between these angles and the former will give the several parallactic angles Psa or Ssa, required for an investigation of the least quantity ma, belonging to every star.

For instance, let the point towards which we may suppose the sun to move, be λ Herculis; and calculating the required angles of the direction in which the effect of parallax will be exerted, with the six stars we have selected for the purpose of our investigation, we find them as in the following Table.

Table III.

Angles of the parallactic Motion with the Parallel.

Sirius	33°	54′	8″,5	south-preceding.
Arcturus	17	23	45,7	south-preceding.
Capella	85	10	3,9	south-following.
Lyra	35	59	49,5	north-following.
Aldebaran	71	21	35,4	south-following.
Procyon	47	43	44,6	south-preceding.

The difference between these parallactic, and the former apparent angles, with the parallel of each star, will give the required angles for our second figure. They will be as follows.

Table IV.

Angles of the apparent with the parallactic Motion.

Sirius	35°	55′	32″,2	south-following.
Arcturus	38	5	56,3	south-following.
Capella	13	34	41,5	south-following.
Lyra	20	21	7,8	north-preceding.
Aldebaran	5	8	1,9	south-preceding.
Procyon	2	18	39,9	south-following.

By these angles, with the assistance of the lines *sa*, whose lengths represent the annual quantity of the apparent motions as given in our former Table, the Figure No. 2 has been constructed. When the situation of these angles is regulated as in that figure, we may draw the several lines *ma* perpendicular to SP, and, by computation, their value and sum will be obtained as follows.

Table V.

Quantities and Sum of the least real Motions.

Sirius	- - -	0,65487
Arcturus	- - -	1,28784
Capella	- - -	0,10887
Lyra	- - -	0,11281
Aldebaran	- - -	0,01104
Procyon	- - -	0,04998
	Sum	2",22491.

The result of this investigation is, that by admitting a motion of the sun towards λ Herculis, the annual proper motions of our six stars, of which the sum is 5",3537, may be reduced to real motions of no more than 2",2249.

When first I proposed λ Herculis as an apex for the solar motion, it may be remembered that a reference to future observations was made for obtaining greater accuracy.* Such observations we have now before us, in the valuable Tables from which I have taken the proper motions of the six stars; and I shall prove that, with their assistance, we may fix on a solar motion that will be considerably more favourable.

We have already shown, that to ascertain the precise place of the best apex is attended with some difficulty; but from the inspection of the figure which represents the quantities of real motion required when λ Herculis is fixed upon, it will be seen that, by a regular method of approximation, we may turn the line SP into a situation where all the angles of the

* See Phil. Trans. for 1783, p. 273, line 8; and page 274, line 4.

apparent motion of the six stars, will be much reduced. The quantities which are required for constructing another figure to represent the threefold motions of our six stars, when a different apex is fixed upon, are to be found by the same method we have pursued in the instance of λ Herculis; and the figure that has been given with respect to that star, shows evidently that the parallactic line SP should be turned more towards the line *1a*, representing the apparent motion of Sirius. We shall accordingly try a point near the following knee of Hercules, whose right ascension is 270° 15′, and north polar distance 54° 45′.

The result of a calculation of the angles and the least quantities of real motion of our six stars, according to this apex, is collected in the following Table, and represented in Fig. 3.

Table VI.

Stars.	Angles of the parallactic Motion with the Parallel.	Angles of the apparent with the parallactic Motion.	Least Quantities of the real Motion.
	° ′ ″	° ′ ″	″
Sirius -	68 51 5 south-preceding	0 1 25 south preceding	0,0004561
Arcturus	29 30 32 south-preceding	25 59 10 south-following	0,9145072
Capella -	77 54 0 south-following	6 18 38 south-following	0,0509727
Lyra -	27 38 47 north-following	28 42 9 north-preceding	0,1557761
Aldebaran	66 20 17 south-following	10 9 21 south preceding	0,0217607
Procyon -	64 48 27 south-following	14 46 1 south-preceding	0,3159053
		Sum	1″,4593779

By this Table it appears that the annual proper motion of our six stars may be reduced to 1″,4594, which is 0″,7655 less than the sum in the 5th Table, where the apex was λ Herculis.

In the approximation to this point it appeared, that when the line of the parallactic motion of Sirius is made to coincide

with its apparent motion, we may soon obtain a certain minimum of the other parallactic motions; but as Sirius is not the star which has the greatest proper motion, it occurred to me that another minimum, obtained from the line in which Arcturus appears to move would be more accurate; for, on account of its great proper motion, we have reason to suppose it more affected than other stars, by the parallax arising from the motion of the sun; and, with a view to this, I soon was led to a point not only in the line of the apparent motion of Arcturus, but equally favourable to Sirius and Procyon, the remaining two stars that have the greatest motions.

If the principle of determining the direction of the solar motion by the stars which have the greatest proper motion be admitted, the following apex must be extremely near the truth; for, an alteration of a few minutes in right ascension or polar distance either way, will immediately increase the required real motion of our stars. Its place is: right ascension 245° 52′ 30″, and north polar distance 40° 22′.

The calculated motions of the same stars by this apex will be as in the following Table, and are delineated in Fig. 4.

Table VII.

Stars.	Angles of the parallactic Motion with the Parallel.				Angles of the apparent with the parallactic Motion.				Least Quantities of the real Motion.
	°	′	″		°	′	″		
Sirius -	58	24	56	south-preceding	10	24	44	following -	0,20157
Arcturus	35	29	45	south-preceding	0	0	3	preceding -	0,00008
Capella -	83	44	17	south-preceding	24	40	21	following -	0,19358
Lyra -	36	28	33	south-following	92	49	30	following -	0,32396
Aldebaran	89	48	35	south-preceding	13	18	58	following -	0,02842
Procyon -	59	43	10	south-preceding	9	40	46	preceding -	0,20839
								Sum	0,95599

The sum of the real motions required, with the apex of the solar motion above mentioned, is less in this Table than that in the former by 0″,50343.

In these calculations we have proceeded upon the principle of obtaining the least possible quantity of real motion, by way of coming at the most favourable situation of a solar apex, and have proved that the sum of the observed proper motions of the six principal stars, amounting to 5″,3334, may be the result of a composition of two other motions, and that the real motions of these stars, if they could be reduced to their smallest possible quantities, would not exceed 0″,9589.

But as I do not intend to assert that these real motions can be actually brought down to the low quantities that have been mentioned, it will be necessary to show that the validity of the arguments for establishing the method I have pursued will not be affected by that circumstance. In the first place then, we should consider that although the great proper motions of Arcturus, Procyon, and Sirius, are strong indications of their being affected by parallax, it does not follow, nor is it probable, that the apparent changes of the situation of these stars should be intirely owing to solar motion; on the contrary, we may reasonably expect that their own real motions will have a great share in them. Next to this, it is evident that in the case of parallactic motions the distance of a star from the sun is of material consequence; and as this cannot be assumed at pleasure, we are consequently not at liberty to make the parallactic motion *sp* in Fig. 1, equal to the line *sm* of the same figure; hence it follows, that the real motion of the star cannot be from *m* to *a*, as the foregoing calculations have supposed; but will be from *p* to *a*. It is however very evident, that if *ma* be

a minimum, the line pa, when sp is given, will also be a minimum; and if all the ma's in Fig. 4 are minima, it follows also that all the sp's, whatever they may be, will give the pa's as small as possible: and this is the point that was to be established.

Whatever therefore may be the sum of real motions required to account for the phenomena of proper motions, our foregoing arguments cannot be affected by the result; for, as by observation it is known that proper motions do exist, and since no solar motion can resolve them intirely into parallactic ones, we ought to give the preference to that direction of the motion of the sun which will take away more real motion than any other, and this, as we have shown, will be done when the right ascension of the apex is 245° 52′ 30″, and its north polar distance 40° 22′.

Fig. 1.

Fig. 2.

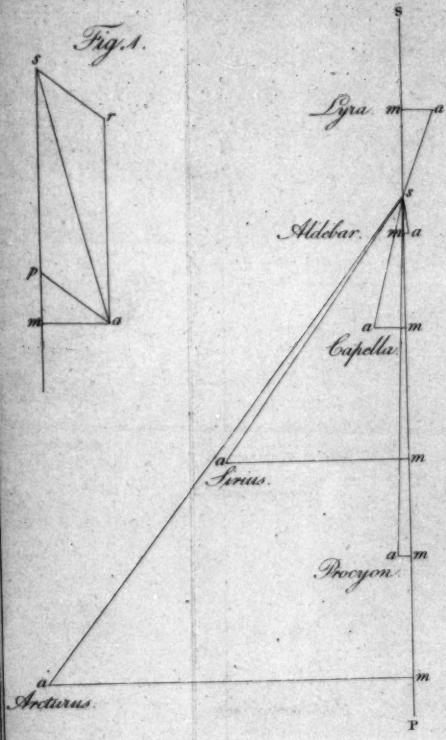


Fig. 3.

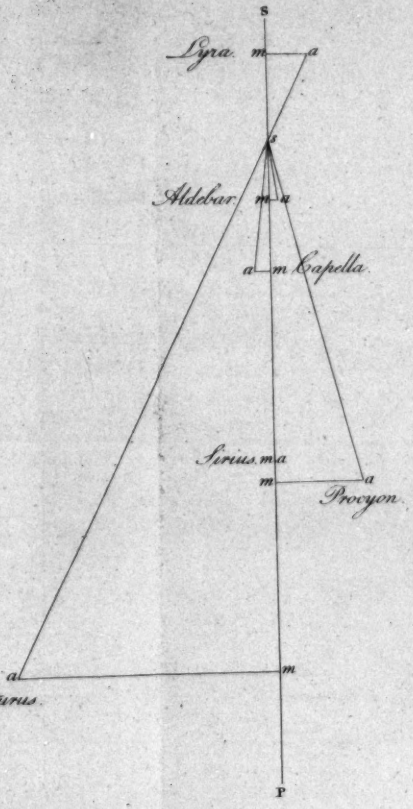
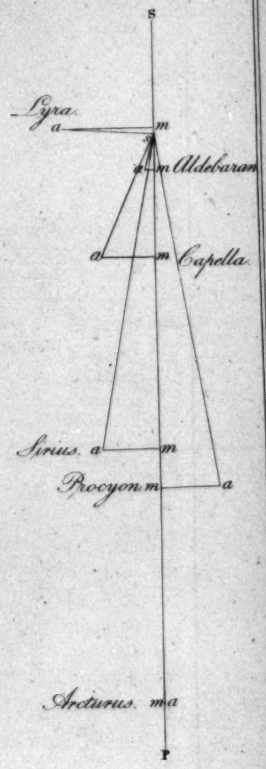


Fig. 4.



Basire fc.

XVII. *On the Reproduction of Buds.* By Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.*

Read May 23, 1805.

MY DEAR SIR,

EVERY tree in the ordinary course of its growth generates, in each season, those buds which expand in the succeeding spring; and the buds thus generated, contain, in many instances, the whole of the leaves which appear in the following summer. But if these buds be destroyed during the winter or early part of the spring, other buds, in many species of trees, are generated, which in every respect perform the office of those which previously existed, except that they never afford fruit or blossoms. This reproduction of buds has not escaped the notice of naturalists; but it does not appear to have been ascertained by them from which, amongst the various substances of the tree, the buds derive their origin.

DU HAMEL conceived that reproduced buds sprang from pre-organized germs; but the existence of such germs has not, in any instance, been proved, and it is well known that the roots, and trunk, and branches, of many species of trees will, under proper management, afford buds from every part of their surfaces; and therefore, if this hypothesis be well founded, many millions of such germs must be annually generated in every large tree; not one of which in the ordinary course of

nature will come into action: and as nature, amidst all its exuberance, does not abound in useless productions, the opinions of this illustrious physiologist are, in this case, probably erroneous.

Other naturalists have supposed the buds, when reproduced, to spring from the plexus of vessels which constitutes the internal bark; and this opinion is, I believe, much entertained by modern botanists: it nevertheless appears to be unfounded, as the facts I shall proceed to state will evince.

If the fruit-stalks of the sea cale *(crambe maritima)* be cut off near the ground in the spring, the medullary substance, within that part of the stalk which remains attached to the root, decays; and a cup is thus formed in which water collects in the succeeding winter. The sides of this cup consist of a woody substance, which in its texture and office, and mode of generation, agrees perfectly with the alburnum of trees; and I conceive it to be as perfect alburnum, as the white wood of the oak or elm: and from the interior part of this substance, within the cup, I have frequently observed new buds to be generated in the ensuing spring. It is sufficiently obvious that the buds in this case do not spring from the bark: but it is not equally evident that they might not have sprung from some remains of the medulla.

In the autumn of 1802, I discovered that the potatoe possessed a similar power of reproducing its buds. Some plants of this species had been set, rather late in the preceding spring, in very dry ground, where through want of moisture they vegetated very feebly; and the portions of the old roots remained sound and entire till the succeeding autumn. Being then moistened by rain, many small tubers were generated on

the surfaces made by the knife in dividing the roots into cuttings; and the buds of these, in many instances, elongated into runners which gave existence to other tubers, some of which I had the pleasure to send to you.

I have in a former Paper remarked, that the potatoe consists of four distinct substances, the epidermis, the true skin, the bark, and its internal substance, which from its mode of formation, and subsequent office, I have supposed to be alburnous: there is also in the young tubes a transparent line through the center, which is probably its medulla. The buds and runners sprang from the substance which I conceive to be the alburnum of the root, and neither from the central part of it, nor from the surface in contact with the bark. It must, however, be admitted, that the internal substance of the potatoe corresponds more nearly with our ideas of a medullary than of an alburnous substance, and therefore this, with the preceding facts, is adduced to prove only that the reproduced buds of these plants are not generated by the cortical substance of the root: and I shall proceed to relate some experiments on the apple, and pear, and plumb-tree, which I conceive to prove that the reproduced buds of those plants do not spring from the medulla.

Having raised from seeds a very considerable number of plants of each of these species in 1802, I partly disengaged them from the soil in the autumn, by digging round each plant, which was then raised about two inches above its former level. A part of the mould was then removed, and the plants were cut off about an inch below the points where the seed-leaves formerly grew; and a portion of the root, about an inch long, without any bud upon it, remained exposed to the air

and light. In the beginning of April, I observed many small elevated points on the bark of these roots, and, removing the whole of the cortical substance, I found that the elevations were occasioned by small protuberances on the surface of the alburnum. As the spring advanced, many minute red points appeared to perforate the bark: these soon assumed the character of buds, and produced shoots, in every respect similar to those which would have sprung from the organized buds of the preceding year. Whether the buds thus reproduced derived any portion of their component parts from the bark or not, I shall not venture to decide; but I am much disposed to believe that, like those of the potatoe, they sprang from the alburnous substance solely.

The space, however, in the annual root, between the medulla and the bark is very small; and therefore it may be contended that the buds in these instances may have originated from the medulla. I therefore thought it necessary to repeat similar experiments on the roots and trunks of old trees, and by these the buds were reproduced precisely in the same manner as the annual roots: and therefore, conceiving myself to have proved in a former Memoir,* that the substance which has been called the medullary process does not originate from the medulla, I must conclude that reproduced buds do not spring from that substance.

I have remarked in a Paper, which you did me the honour to lay before the Royal Society in the commencement of the present year, that the alburnous tubes at their termination upwards invariably join the central vessels, and that these vessels, which appear to derive their origin from the alburnous tubes, convey nutriment, and probably give existence to new

* Phil. Trans. of 1803.

buds and leaves. It is also evident, from the facility with which the rising sap is tranferred from one side of a wounded tree to the other, that the alburnous tubes possess lateral, as well as terminal, orifices: and it does not appear improbable that the lateral as well as the terminal orifices of the alburnous tubes may possess the power to generate central vessels; which vessels evidently feed, if they do not give existence to, the reproduced buds and leaves. And therefore, as the preceding experiments appear to prove that the buds neither spring from the medulla nor the bark, I am much inclined to believe that they are generated by central vessels which spring from the lateral orifices of the alburnous tubes. The practicability of propagating some plants from their leaves may seem to stand in opposition to this hypothesis; but the central vessel is always a component part of the leaf, and from it the bud and young plant probably originate.

I expected to discover in seeds a similar power to regenerate their buds; for the cotyledons of these, though dissimilar in organization, execute the office of the alburnum, and contain a similar reservoir of nutriment, and at once supply the place of the alburnum and the leaf. But no experiments, which I have yet been able to make, have been decisive, owing to the difficulty of ascertaining the number of buds previously existing within the seed. Few, if any, seeds, I have reason to believe, contain less than three buds, one only of which, except in cases of accident, germinates; and some seeds appear to contain a much greater number. The seed of the peach appears to be provided with ten or twelve leaves, each of which probably covers the rudiment of a bud, and the seeds, like the buds of the horse-chesnut, contain all the leaves and apparently

all the buds of the succeeding year: and I have never been able to satisfy myself that all the buds were eradicated without having destroyed the base of the plumule, in which the power of reproducing buds probably resides, if such power exists.

Nature appears to have denied to annual and biennial plants (at least to those which have been the subjects of my experiments) the power which it has given to perennial plants to reproduce their buds; but nevertheless some biennials possess, under peculiar circumstances, a very singular resource, when all their buds have been destroyed. A turnip, bred between the English and Swedish variety, from which I had cut off the greater part of its fruit-stalks, and of which all the buds had been destroyed, remained some weeks in an apparently dormant state; after which the first seed in each pod germinated, and bursting the seed-vessel, seemed to execute the office of a bud and leaves to the parent plant, during the short remaining term of its existence, when its preternatural foliage perished with it. Whether this property be possessed by other biennial plants in common with the turnip or not, I am not at present in possession of facts to decide, not having made precisely the same experiment on any other plant.

I will take this opportunity to correct an inference that I have drawn in a former Paper,* which the facts (though quite correctly stated) do not, on subsequent repetition of the experiment, appear to justify. I have stated, that when a perpendicular shoot of the vine was inverted to a depending position, and a portion of its bark between two circular incisions round the stem removed, much more new wood was generated on the lower lip of the wound become uppermost by the

* Phil. Trans of 1803.

inverted position of the branch, than on the opposite lip, which would not have happened had the branch continued to grow erect, and I have inferred that this effect was produced by sap which had descended by gravitation from the leaves above. But the branch was, as I have there stated, employed as a layer, and the matter which would have accumulated on the opposite lip of the wound had been employed in the formation of roots, a circumstance which at that time escaped my attention. The effects of gravitation on the motion of the descending sap, and consequent growth of plants, are, I am well satisfied, from a great variety of experiments, very great; but it will be very difficult to discover any method by which the extent of its operation can be accurately ascertained. For the vessels which convey and impel* the true sap, or fluid from which the new wood appears to be generated, pass immediately from the leaf-stalk towards the root; and though the motion of this fluid may be impeded by gravitation, and it be even again returned into the leaf, no portion of it, unless it had been extravasated, could have descended to the part from which the bark was taken off in the experiment I have described. I am not sensible that in the different Papers which I have had the honour to address to you, I have drawn any other inference which the facts, on repetition of the experiments, do not appear capable of supporting.

I am, &c.

THO^{S}. ANDREW KNIGHT.

Elton,
May 12, 1805.

* Phil. Trans. of 1804.

XVIII. *Some Account of two Mummies of the Egyptian Ibis, one of which was in a remarkably perfect State.* By John Pearson, *Esq. F. R. S.*

Read June 13, 1805.

The ancient Egyptians were not more remarkable for their attainments in science, than for the extraordinary attention they paid to the bodies of their deceased relatives, preserving their remains by arts which are now either unknown, or imperfectly recorded, and depositing them in subterranean structures, which to this day excite the curiosity and wonder of the philosophic traveller. The practice of embalming was not confined, as is well known, to the conservation of human bodies exclusively; it was likewise employed to protect the remains of several of their sacred animals from that decay and dissolution which usually ensues, on the exposure of animal substances to the action of the earth, or of the atmosphere. We learn from Herodotus,* that among the different animals which the Egyptians honoured with this peculiar mode of sepulture, were the cat, the ichneumon, the mus araneus terrestris, the ibis, and the hawk; but, whether this be a complete enumeration or not, it is almost impossible, at this period of time, to determine. Mummies of the hawk and of the ibis have been often drawn out of the catacombs; and Olivier asserts, that he has not only met with the bones of the mus

* Euterpe.

araneus terrestris, but also with those of several of the smaller species of quadrupeds, and that the bones of different animals are not unfrequently contained within the same wrapper.* It is however confidently affirmed by different writers, that the more modern Egyptians have frequently included a single bone of some quadruped within the usual quantity of cloth, which they have artfully taken from some decayed mummy in the catacombs, and then fraudulently sold this sophisticated production as an ancient mummy. Hence, any general conclusions founded on meeting with the bones of other quadrupeds, must be received with diffidence and suspicion.†

The mummies which are taken out of the catacombs of the birds at Saccara, and at Thebes, are included in earthen jars, closed with a cover of the same material. The cloth which envelopes the mummy is sometimes tolerably firm and perfect; but, on removing this, we commonly meet with a quantity of dust, resembling powdered charcoal in its appearance, intermixed with the bones, or the fragments of bones, belonging to the creature which had been contained in it. The decomposition is often so complete, that no traces of the animal remain; but, on other occasions, the intire collection of bones, with the bill of the bird, have been found in a condition sufficiently perfect to construct a skeleton with them. In the fourth volume of the *Annales du Mustum National d'Histoire naturelle*, M. CUVIER has published an interesting memoir on the Ibis, with an engraving of the skeleton of that bird, which had been formed of the bones collected from the catacombs at Thebes. That able naturalist, after comparing the ancient accounts of that celebrated bird with those of the moderns,

* *Voyage en Egypte*, Tome III. chap. viii. † Phil. Trans. 1794.

assigns it a place among the species of curlew, under the name of Numenius Ibis.

The accounts of the mummy of the ibis which have been hitherto made public, were collected from observations made on it in a decayed state: I presume, therefore, that a description of the mummy of an ibis in a condition unusually perfect, may not be unacceptable to the curious. Among the curiosities, natural and artificial, which were collected by the late Major Hayes,* in the years 1802 and 1803, were two small mummies, which he took out of the catacombs at Thebes in Upper Egypt. They were contained in earthen jars, and were enveloped in cloth, similar to those which are brought from Saccara. At the request of his family, I first examined the larger of the two, and found the covering to consist of bandages of cloth, strong and firm, and about three inches broad. The first circumvolutions of the roller separated easily; but, as I proceeded, they adhered more firmly to each other, and were at length so closely cemented together by a resinous-like substance, that I was obliged to divide the folds of the cloth with a strong knife. Each layer of the bandage appeared to have been imbued with some bituminous or resinous substance, in a liquid state, and the roller was farther secured by strong pieces of thread, so that the whole mass was rendered extremely hard and coherent. When I had removed the greater part of the covering, I found that it had contained a bird, which was thickly covered with the same kind of substance that had cemented the different strips of the roller. The examination

* This accomplished young gentleman, who served during the late campaign in Egypt, died July 26, 1803, at Rosetta, aged 25 years. By his premature death, his country lost an able officer, and a zealous promoter of the interests of science.

was now carried on more slowly, by picking out carefully all the loose bituminous matter that could be removed without injuring the mummy; and, after the labour of many hours, I succeeded in displaying the whole bird, as it had been deposited by the embalmer. The operator who had embalmed this bird, had previously disposed its several parts with great order and regularity.

The neck was twisted, so as to place the vertex of the head on the body of the bird, a little to the left side of the sternum. The curved bill, with its concave part turned upwards, descended between the feet, and reached to the extremity of the tail. Each foot, with its four claws turned forwards, was bent upwards, and placed on each side of the head. The wings were brought close to the sides of the body. It was impossible to remove much of the bituminous matter from the back and wings, without injuring the mummy; but I took away a quantity sufficient to show that the plumage was white, the feathers being tipped with dark brown at their extremities; I could not, however, uncover the tail feathers, so as to determine their colour. The bird had attained its full growth; for the quills of one wing, which had suffered some injury in removing the bandage, were in a perfect state: the largest of these quills is delineated, of the natural size, in the annexed Plate. The following are the dimensions of such parts of the Ibis as are accessible.

	Inches.
Length of the bird, from the termination of the neck to the extremity of the tail - - - - -	12½
Length of the neck, in which ten vertebræ can be traced - - - - - - -	6½
Length of the head and bill, following the curve -	8

	Inches.
Length of the sternum	4
From the end of the metatarsal bone to the extremity of the longest toe	7
The longest toe	8½
Width of the body at the shoulders	4½
Circumference of the body, at its thickest part	18½

Weight of the mummy, 16½ ounces Troy.

This mummy is in a very firm and intire state, exhibiting no particular marks of decay, although it is probable, that the greater part of 3000 years has elapsed since it was interred; for the destruction of the Egyptian Thebes is of an earlier date than the foundation of any city now existing. The appearance of the mummy renders it probable, that the bird was immersed in the bituminous matter, when it was in a liquid state, and capable of insinuating itself into all the inequalities on the surface of the body; the several folds of the bandage must have been likewise covered with the same varnish: but the animal was certainly not boiled in the liquid, as GREW supposed,* since the feathers are not at all corrugated, nor indeed materially changed from their natural appearance.

The examination of different mummies of the Ibis proves indubitably that the same care has not been used, nor have the same methods been followed, in the preparing of them; but, whether the difference observed depended upon the condition of the bird when it was embalmed, or upon the unequal skill and diligence of the operators, cannot now be ascertained. This, however, is sufficiently evident, that the variety exhibited in their appearance does not depend on the place where the bird was deposited, since many mummies of birds have been taken

* *Museum Regalis Societatis*, § 1.

from the catacombs at Thebes, in as imperfect and decayed a condition as those which have been procured from Sactara.

I have been favoured with the permission to unroll another mummy of the Ibis, also sent from Thebes by Major HAYES, which had been embalmed in a different manner from that I have already described. The cloth which surrounded it was of a coarser texture, and had not been so thoroughly imbued with bitumen, nor was the roller continued down to the body of the bird; for, when I had removed as much of the bandage as reduced the mummy to about $\frac{1}{3}$ of its original bulk, I found that, instead of circular bands, it was wrapped in several different portions of coarse linen cloth, each of them large enough to contain the whole Ibis. This Ibis was in a decayed state, and had so little coherence, that its several parts separated on handling it: there was a small portion of the neck, with white plumage upon it, remaining, but neither the head, the bill, nor any remains of them, could be discovered. The feathers of this bird are of a dark brown colour, in some parts tipped with white; the neck and the tail have a white plumage, and as much of the tail as could be preserved displayed the tufted appearance delineated in the engraving of M. CUVIER.

Two species of the Ibis, the black and the white, have been noticed by HERODOTUS,* ARISTOTLE,† and PLINY:‡ but PLUTARCH has only mentioned the white Ibis.§ ARISTOTLE and PLINY have contended that the black Ibis was found only at Damietta, (Pelusium,) and that, in all the other parts of Egypt, the white Ibis only was seen. Whether the two birds which I

* Euterpe.

† *Hist. Animalium*, lib. ix. c. xxvii.

‡ C. PLINII *Nat. Hist.* lib. x. c. xxx.

§ *De Iside et Osiride.*

have described present specimens of the black and white Ibis, I cannot presume to determine. The anterior layer of feathers of the Ibis last examined is of a dark colour; but the plumage beneath is white. Many of the dark feathers are not at all marked with white.

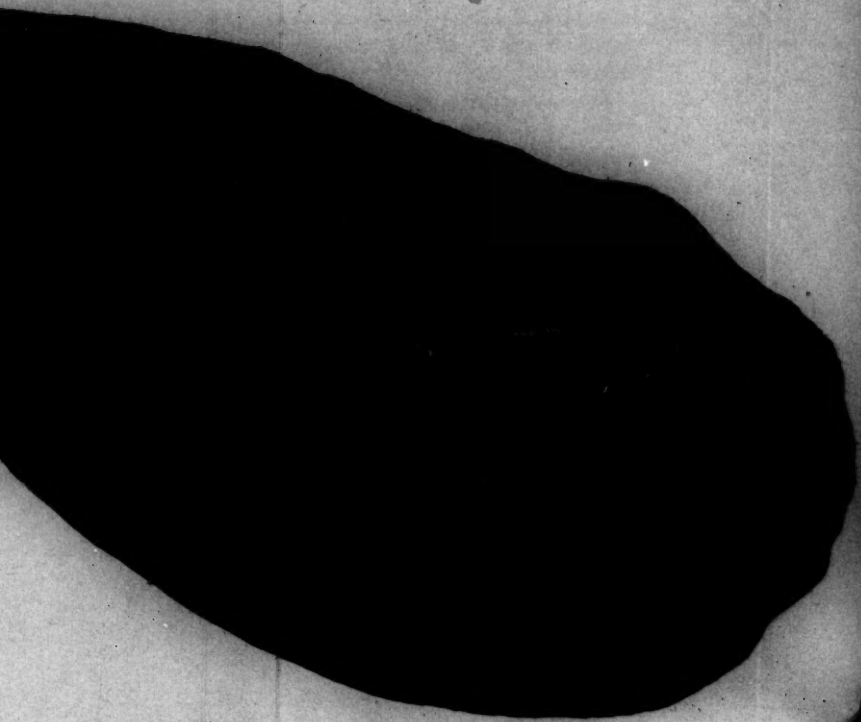
The most ancient, and probably the most authentic account which we possess of the Egyptian art of embalming, is delivered by HERODOTUS;* and what is offered upon this subject by subsequent writers, seems to have been copied from this early historian. Their narratives relate principally to the conservation of human bodies; and, in the preparing of these, it appears that the contents of the abdomen, at least, were removed by incision, or were corroded by injecting a liquor extracted from the cedar-tree.† But it is almost certain, that birds were not previously opened, nor was any art employed to remove the stomach and intestines; for, on examining the interior parts of the dark coloured Ibis, I met with a soft spongy substance, lying quite loose, containing a great number of scarabæi in an imperfect state; these had probably been taken as the food of the bird, and were not digested at the time of its death, but remained in the alimentary canal to the present period. CUVIER also remarks, that he found within the mummy of an Ibis part of the skin and scales of a serpent.

As larvæ of dermestides and other insects have been detected among the dust and bones of a mummy, it may be presumed that the Ibis was not always embalmed in a fresh state; which may indeed account, in part, for the very imperfect condition in which many of these birds are found.

The Ibis was held in great veneration by the Egyptians for

* Euterpe. † PANCIROLLUS *Rerum memorab.* pars 1. tit. xii.

W. Clift del.



its singular utility in destroying serpents, and other noxious reptiles:* hence, the figure of this bird is seen on many monuments of Egyptian antiquity, as an inhabitant of their temples, and an attendant on their sacrifices.† It was likewise employed as a symbol in their hieroglyphical writing;‡ and the punishment of death was inflicted on those who killed this sacred bird. The other extraordinary qualities ascribed to the Ibis by PLINY, PLUTARCH, and some succeeding writers, are either too indistinctly expressed to be quite intelligible, or too obviously absurd to be credible.§

EXPLANATION OF PLATE VIII.

A, Vertebræ of the neck.
B, The head.
C, The bill.
D, The tail.
E, The right leg and foot.
F, The left leg and foot.
G, The hind claw bent forwards.
H, The sternum.
I, A quill of the wing feathers.
The whole is represented of the natural size.

* The remark of CICERO on this subject, is perhaps no less true than shrewd: "Ipsi, qui irridentur, Egyptii, nullam belluam, nisi ob aliquam utilitatem, quam ex "ea caperent, consecraverunt." *De Natura Deorum*, lib. i.

† *Explication des divers Monumens singuliers*, CALMET.

‡ *Hieroglyph. Horapollo*, XXXVI. RHODIGIN. *Antiq. Lect.* lib. iv. c. xvi.

§ C. PLINII *Nat. Hist.* lib. viii. c. XXVII. PLUTARCH. *De Iside*, &c.

XIX. *Observations on the singular Figure of the Planet Saturn.* By William Herschel, *LL. D. F. R. S.*

Read June 20, 1805.

THERE is not perhaps another object in the heavens that presents us with such a variety of extraordinary phenomena as the planet Saturn: a magnificent globe, encompassed by a stupendous double ring: attended by seven satellites: ornamented with equatorial belts: compressed at the poles: turning upon its axis: mutually eclipsing its ring and satellites, and eclipsed by them: the most distant of the rings also turning upon its axis, and the same taking place with the farthest of the satellites: all the parts of the system of Saturn occasionally reflecting light to each other: the rings and moons illuminating the nights of the Saturnian: the globe and satellites enlightening the dark parts of the rings: and the planet and rings throwing back the sun's beams upon the moons, when they are deprived of them at the time of their conjunctions.

It must be confessed that a detail of circumstances like these, appears to leave hardly any room for addition, and yet the following observations will prove that there is a singularity left, which distinguishes the figure of Saturn from that of all the other planets.

It has already been mentioned on a former occasion, that so far back as the year 1776 I perceived that the body of Saturn was not exactly round; and when I found in the year 1781

that it was flattened at the poles at least as much as Jupiter, I was insensibly diverted from a more critical attention to the rest of the figure. Prepossessed with its being spheroidical, I measured the equatorial and polar diameters in the year 1789, and supposed there could be no other particularity to remark in the figure of the planet. When I perceived a certain irregularity in other parts of the body, it was generally ascribed to the interference of the ring, which prevents a complete view of its whole contour; and in this error I might still have remained, had not a late examination of the powers of my 10-feet telescope convinced me that I ought to rely with the greatest confidence upon the truth of its representations of the most minute objects I inspected.

The following observations, in which the singular figure of Saturn is fully investigated, contain many remarks on the rest of the appearances that may be seen when this beautiful planet is examined with attention; and though they are not immediately necessary to my present subject, I thought it right to retain them, as they show the degree of distinctness and precision of the action of the telescope, and the clearness of the atmosphere at the time of observation.

April 19, 1805. With a new 7-feet mirror of extraordinary distinctness, I examined the planet Saturn. The ring reflects more light than the body, and with a power of 570 the colour of the body becomes yellowish, while that of the ring remains more white. This gives us an opportunity to distinguish the ring from the body, in that part where it crosses the disk, by means of the difference in the colour of the reflected light. I saw the quintuple belt, and the flattening of the body at the

polar regions; I could also perceive the vacant space between the two rings.

The flattening of the polar regions is not in that gradual manner as with Jupiter, it seems not to begin till at a high latitude, and there to be more sudden than it is towards the poles of Jupiter. I have often made the same observation before, but do not remember to have recorded it any where.

April 18; 10-feet reflector, power 300. The air is very favourable, and I see the planet extremely well defined. The shadow of the ring is very black in its extent over the disk south of the ring, where I see it all the way with great distinctness.

The usual belts are on the body of Saturn; they cover a much larger zone than the belts on Jupiter generally take up, as may be seen in the figure I have given in Plate IX.; and also in a former representation of the same belts in 1794.*

The figure of the body of Saturn, as I see it at present, is certainly different from the spheroidical figure of Jupiter. The curvature is greatest in a high latitude.

I took a measure of the situation of the four points of the greatest curvature, with my angular micrometer, and power 527. When the cross of the micrometer passed through all the four points, the angle which gives the double latitude of two of the points, one being north the other south of the ring, or equator, was 93° 16′. The latitude therefore of the four points is 46° 38′; it is there the greatest curvature takes place. As neither of the cross wires can be in the parallel, it makes the measure so difficult to take, that very great accuracy cannot be expected.

* See Phil. Trans. for 1794, Table VI. page 32.

The most northern belt comes up to the place where the ring of Saturn passes behind the body, but the belt is bent in a contrary direction being concave to the north, on account of its crossing the body on the side turned towards us, and the north pole being in view.

There is a very dark, but narrow shadow of the body upon the following part of the ring, which as it were cuts off the ring from the body.

The shadow of the ring on the body, which I see south of the ring, grows a little broader on both sides near the margin of the disk.

The division between the two rings is dark, like the vacant space between the ansæ, but not black like the shadow I have described.

There are four satellites on the preceding side near the ring; the largest and another are north-preceding; the other two are nearly preceding.

April 19. I viewed the planet Saturn with a new 7-feet telescope, both mirrors of which are very perfect. I saw all the phenomena as described last night, except the satellites, which had changed their situation; four of them being on the following side. This telescope however is not equal to the 10-feet one.

The remarkable figure of Saturn admits of no doubt: when our particular attention is once drawn to an object, we see things at first sight that would otherwise have escaped our notice.

10-feet reflector, power 400. The night is beautifully clear, and the planet near the meridian. The figure of Saturn is somewhat like a square or rather parallelogram, with the four

corners rounded off deeply, but not so much as to bring it to a spheroid. I see it in perfection.

The four satellites that were last night on the preceding, are now on the following side, and are very bright.

I took a measure of the position of the four points of the greatest curvature, and found it 91° 29′. This gives their latitude 45° 44′.5. I believe this measure to be pretty accurate. I set first the fixed thread to one of the lines, by keeping the north-preceding and south-following two points in the thread; then adjusted the other thread in the same manner to the south-preceding and north-following points.

May 5, 1805. I directed my 20-feet telescope to Saturn, and, with a power of about 300, saw the planet perfectly well defined, the evening being remarkably clear. The shadow of the ring on the body is quite black. All the other phenomena are very distinct.

The figure of the planet is certainly not spheroidical, like that of Mars and Jupiter. The curvature is less on the equator and on the poles than at the latitude of about 45 degrees. The equatorial diameter is however considerably greater than the polar.

In order to have the testimony of all my instruments, on the subject of the structure of the planet Saturn, I had prepared the 40-feet reflector for observing it in the meridian. I used a magnifying power of 360, and saw its form exactly as I had seen it in the 10 and 20-feet instruments. The planet is flattened at the poles, but the spheroid that would arise from this flattening is modified by some other cause, which I suppose to be the attraction of the ring. It resembles a parallelogram, one side whereof is the equatorial, the other the polar diameter,

with the four corners rounded off so as to leave both the equatorial and polar regions flatter than they would be in a regular spheroidical figure.

The planet Jupiter being by this time got up to a considerable altitude, I viewed it alternately with Saturn in the 10-feet reflector, with a power of 500. The outlines of the figure of Saturn are as described in the observation of the 40-feet telescope; but those of Jupiter are such as to give a greater curvature both to the polar and equatorial regions than takes place at the poles or equator of Saturn which are comparatively much flatter.

May 12. I viewed Saturn and Jupiter alternately with my large 10-feet telescope of 24 inches aperture; and saw plainly that the former planet differs much in figure from the latter. The temperature of the air is so changeable that no large mirror can act well.

May 13. 10-feet reflector, power 300. The shadows of the ring upon the body, and of the body upon the ring, are very black, and not of the dusky colour of the heavens about the planet, or of the space between the ring and planet, and between the two rings. The north-following part of the ring, close to the planet, is as it were cut off by the shadow of the body; and the shadow of the ring lies south of it, but close to the projection of the ring.

The planet is of the form described in the observation of the 40-feet telescope; I see it so distinctly that there can be no doubt of it. By the appearance, I should think the points of the greatest curvature not to be so far north as 45 degrees.

The evening being very calm and clear, I took a measure

of their situation, which gives the latitude of the greatest curvature 45° 21′. A second measure gives 45° 41′.

Jupiter being now at a considerable altitude, I have viewed it alternately with Saturn. The figure of the two planets is decidedly different. The flattening at the poles and on the equator of Saturn is much greater than it is on Jupiter, but the curvature at the latitude of from 40 to 48° on Jupiter is less than on Saturn.

I repeated these alternate observations many times, and the oftener I compared the two planets together, the more striking was their different structure.

May 26. 10-feet reflector. With a parallel thread micrometer and a magnifying power of 400, I took two measures of the diameter of the points of greatest curvature. A mean of them gave 64,3 divisions = 11″,98. After this, I took also two measures of the equatorial diameter, and a mean of them gave 60,5 divisions = 11″,27; but the equatorial measures are probably too small.

To judge by a view of the planet, I should suppose the latitude of the greatest curvature to be less than 45 degrees. The eye will also distinguish the difference in the three diameters of Saturn. That which passes through the points of the greatest curvature is the largest; the equatorial the next, and the polar diameter is the smallest.

May 27. The evening being very favourable, I took again two measures of the diameter between the points of greatest curvature, a mean of which was 63,8 divisions = 11″,88. Two measures of the equatorial diameter gave 61,9 divisions = 11″,44.

June 1. It occurred to me that a more accurate measure might be had of the latitude in which the greatest curvature takes place, by setting the fixed thread of the micrometer to the direction of the ring of Saturn, which may be done with great accuracy. The two following measures were taken in this manner, and are more satisfactory than I had taken before. The first gave the latitude of the south-preceding point of greatest curvature 43° 26′; and the second 43° 13′. A mean of the two will be 43° 20′.

June 2. I viewed Jupiter and Saturn alternately with a magnifying power of only 300, that the convexity of the eye-glass might occasion no deception, and found the form of the two planets to differ in the manner that has been described.

With 200 I saw the difference very plainly; and even with 160 it was sufficiently visible to admit of no doubt. These low powers show the figure of the planets perfectly well, for as the field of view is enlarged, and the motion of the objects in passing it lessened, we are more at liberty to fix our attention upon them.

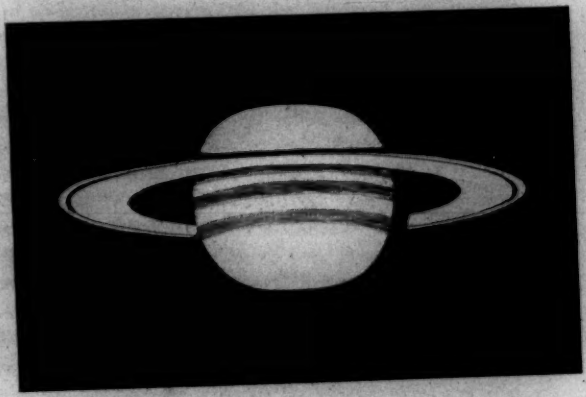
I compared the telescopic appearance of Saturn with a figure drawn by the measures I have taken, combined with the proportion between the equatorial and polar diameters determined in the year 1789;* and found that, in order to be a perfect resemblance, my figure required some small reduction of the longest diameter, so as to bring it nearly to agree with the measures taken the 27th of May. When I had made the necessary alteration, my artificial Saturn was again compared with the telescopic representation of the planet, and I was then satisfied that it had all the correctness of which a judgment of

* See Phil. Trans. for 1790, page 17.

the eye is capable. An exact copy of it is given in Plate IX. The dimensions of it in proportional parts are,

The diameter of the greatest curvature	96
The equatorial diameter	85
The polar diameter	89
Latitude of the longest diameter	48° 20′

The foregoing observations of the figure of the body of Saturn will lead to some intricate researches, by which the quantity of matter in the ring, and its solidity, may be in some measure ascertained. They also afford a new instance of the effect of gravitation on the figure of planets; for in the case of Saturn, we shall have to consider the opposite influence of two centripetal and two centrifugal forces: the rotation of both the ring and planet having been ascertained in some of my former Papers.



Basire sc.

XX. *On the magnetic Attraction of Oxides of Iron.* By Timothy Lane, *Esq. F. R. S.*

Read June 20, 1805.

HAVING found by experiment, that hardened iron is not so readily attracted by the magnet as soft iron, and that needles are inferior to iron wire as indexes to Six's thermometer, I was proceeding to other comparative experiments, when I received the Second Part of last year's Philosophical Transactions, in which I saw an Analysis of magnetical Pyrites, with Remarks on Sulphurets of Iron, by Mr. HATCHETT.

This Paper led me to examine what magnetical properties iron possessed, when free from inflammable matter. For this purpose I obtained a precipitate of iron, prepared and sold at Apothecaries' Hall by the name of *Ferrum precipitatum*. Mr. MOORE, the chemical operator, informed me, that he prepared it by dissolving twelve pounds of sulphate of iron in twenty-four gallons of distilled water, and then adding eight ounces of sulphuric acid to render the solution more complete. Twelve pounds of purified kali were mixed with the solution: the precipitate was well washed with hot distilled water, and then carefully dried. This precipitate is similar to the sediment of chalybeate waters, and affords no magnetic particles; nor, when exposed to a continued clear red heat, does it suffer any alteration beyond the acquirement of a darker colour. But if any smoke or flame has access to it, then magnetic particles

are evident. Heat, by the converging rays of the sun,* equal to that at which glass melts, blackens the oxide, but does not render it magnetic, if free from any inflammable matter. It is requisite, in this experiment, to protect the oxide, by glass, from the dust floating in the air, which otherwise will render many of the particles magnetic. I attributed this effect to the deoxidising property of light, till by employing a protecting glass, the result proved it to proceed from the dust in the atmosphere.

By repeated experiments I found, that heat alone produced no magnetic effect on the oxide, and that inflammable matter with heat always rendered some of the particles magnetic.

As the inflammable matter in coal had this effect, I mixed some of the oxide with a portion of coal in a glass mortar, and continued rubbing them together for some time without any magnetic effect. The mixture was then put into a tobacco-pipe, and placed in the clear red heat of a common fire; as soon as the pipe had acquired a red heat, it was taken out. The mixture was put on a glazed tile to cool, and proved highly magnetic.

I rubbed a portion of the original oxide in a glass mortar with a variety of substances, as sulphur, charcoal, camphor, ether, alcohol, &c. and found that no effect was produced without the assistance of heat. The heat of boiling water, moreover, was not sufficient; but by the heat of melting lead I procured magnetism. Small quantities of any inflammable matter in a red heat have an evident effect on the oxide. Hydrogen, aided by a red heat, renders the oxide magnetic.

* The lens employed in this experiment was twelve inches in diameter, and the heat at its focus was sufficient to melt iron: from Mr. DOLLOND.

Alcohol has the same effect. But if the alcohol be diluted with water, though it may flame in the fire, it will be ineffectual, as it is driven off before the oxide becomes sufficiently heated to receive its action.

Such combustible substances, as do not very readily part with their carbonic element, require rather longer continuance of heat than others: for example, charcoal and cinders, well burnt, must be longer in the fire to have their full effect on the oxide, than dry wood, coal, or sulphur.

But such substances as may be sublimed with facility, will gradually quit the oxide, by a continued application even of a low heat, leaving it unmagnetic, as at first.

How very small a portion of inflammable matter is requisite to render a considerable quantity of oxide magnetic, is evident, since one grain of camphor dissolved in an adequate portion of alcohol, and mixed with a hundred grains of the oxide in a glass mortar, will, by a red heat, render all the particles of the oxide magnetic.

As oxides of iron therefore are rendered magnetic by heat, when mixed with inflammable matter, it may be understood why Prussian blue, sulphurets, and ores of iron containing inflammable matter, become magnetic by the agency of fire; while at the same time it is observable, that these same ores revert to their unmagnetic state, when the heat has been continued sufficiently long to drive off the whole of the inflammable matter: thus we find among the cinders of a common fire calcined sulphurets of iron, distinguishable by their red colour, unmagnetic when all the sulphur is sublimed.

My intention in this communication is to prove generally that mere oxides of iron are not magnetic; that any inflam-

mable substances mixed with them do not render them magnetic, until they are by heat chemically combined with the oxides, and that when the combustible substance is again separated by heat, the oxides return to their unmagnetic state. That magnetic oxides cannot be distinguished from calcined oxides by their colour. I entertain a hope, however, that this subject may be found worthy of the accurate investigation of some other member of this learned Society.

XXI. *Additional Experiments and Remarks on an artificial Substance, which possesses the principal characteristic Properties of Tannin.* By Charles Hatchett, *Esq. F. R. S.*

Read June 27, 1805.

§ 1.

When I had ascertained that carbonaceous substances, whether vegetable, animal, or mineral, were capable of being converted into a product, which, by its effects on earthy and metallic solutions, on dissolved gelatine, and on skin, resembled the natural vegetable principle called *Tannin*, I was at first inclined to give it the name of artificial or factitious *tannin*; but some eminent chemists of this country, for whose opinions I have the highest respect, considered this name as objectionable; for although the artificial substance resembles *tannin* in the particulars above stated, yet in one character there appears to be a very considerable difference, namely, the effect of nitric acid; for by this, the artificial substance is *produced*, whilst the varieties of natural *tannin* are *destroyed*. Such an objection, sanctioned by such authority, induced me to alter the title of my Paper, and to expunge the word *tannin* wherever it had been applied to the artificial product.

In order to satisfy myself more fully on this point, I have, since the communication of my former Paper, made a few experiments on the comparative effects produced by nitric acid

on those substances which contain the most notable quantities of *tannin*, and of these I shall now give a succinct account, and shall also cursorily notice other experiments, in which a tanning substance has been produced, under circumstances different, in some measure, from those which have been already described.

§ II.

Although I cannot as yet assert, that the artificial tanning product is absolutely indestructible when repeatedly distilled with different portions of nitric acid, yet the following experiments will prove, that the destructibility of it by this method must at least be a work of considerable time and difficulty.

1. Twenty grains of this substance were dissolved in half an ounce of strong nitric acid, the specific gravity of which was 1.40. The solution was then subjected to distillation until the whole of the acid had come over, after which, it was poured back upon the residuum, and the distillation was thus repeated three times.

Care was taken not to overheat the residuum, and this, when examined, did not appear to have suffered alteration in any of its properties.

2. Ten grains of the artificial tanning substance, mixed with ten grains of white sugar, were dissolved in half an ounce of nitric acid, and the whole was distilled to dryness.

The residuum being then dissolved in boiling distilled water, and examined by solution of gelatine and other reagents, was found to be unchanged in every respect.

3. This resembled the former, only that gum arabic was employed in the place of sugar. The result was the same.

4. A quantity of dissolved isinglass was precipitated by a

solution of the artificial tanning substance, and the precipitate having been well washed with hot distilled water, was afterwards gradually dried. It was then digested in strong nitric acid, which after some time acted powerfully upon it; much nitrous gas was evolved, and a dark brown solution was formed. This was evaporated to dryness, and after having been completely dissolved in boiling distilled water, was examined by nitrate of lime, acetite of lead, muriate of tin, and solution of isinglass, all of which formed copious precipitates, similar in every respect to those produced by the artificial tanning substance, which had not been subjected to the above described process.

5. A portion of the precipitate, formed by isinglass and the tanning substance, was dissolved in pure muriatic acid, and was afterwards evaporated to dryness. Boiling distilled water dissolved only a small part, and the solution, which was of a dark beer colour, did not precipitate gelatine, although it acted upon muriate of tin, and sulphate of iron; for with the former it produced an ash-coloured precipitate, and with the latter a slight deposit of a reddish-brown colour.

6. As so small a part of the precipitated isinglass had been thus rendered soluble in boiling water, the residuum was treated with nitric acid, as in experiment 4, after which, being evaporated to dryness, it was found to be completely soluble in water, and precipitated gelatine as copiously as at first.

7. I dissolved 20 grains of the pure tanning substance in about half an ounce of muriatic acid; but, after distillation to dryness, the residuum in every respect appeared to be unchanged.

In addition to the above experiments may be added, that the solutions of the artificial tanning substance seem to be completely imputrescible, neither do they ever become mouldy like the infusions of galls, sumach, catechu, &c.

Having thus ascertained the very unchangeable nature of this substance, I made the following comparative experiments on galls, sumach, Pegu cutch, kascutti, common cutch, and oak bark.

8. Twenty grains of powdered galls were dissolved in half an ounce of the strong nitric acid; the solution was then evaporated to dryness, and the residuum dissolved in boiling water. This did not produce the smallest effect on dissolved gelatine.

9. A strong infusion of galls evaporated to dryness, and treated as above, was totally deprived of the tanning property.

10. Isinglass precipitated by the infusion of galls, was dissolved in nitric acid, and examined as in experiment 4, but no trace of *tannin* could be discovered.

11. Twenty grains of sumach were dissolved in half an ounce of the strong nitric acid, and treated as in experiment 8, after which it appeared that the *tannin* was destroyed.

12. Twenty graints of Pegu cutch (which contains a considerable quantity of mucilage) were subjected to a similar process, by which much oxalic acid was obtained, but every vestige of *tannin* was obliterated.

13. Twenty grains of the catechu called Kascutti afforded results similar to the above.

14. Twenty grains of the common cutch or catechu being dissolved in nitric acid, evaporated to dryness, dissolved in water, and examined by solution of isinglass, rendered the latter

turbid, a tenacious film was deposited, which was insoluble in boiling water, and was evidently composed of gelatine and *tannin*.

15. Twenty grains of prepared oak bark, by the like treatment, afforded a solution in water, which still acted in some measure upon gelatine, as it caused a solution of isinglass to become slightly turbid, and a film completely insoluble in boiling water was, as in experiment 14, deposited on the sides and bottom of the vessel.

16. Infusions were prepared as nearly as possible of equal strength from galls, sumach, shavings of oakwood, oak bark, and the artificial tanning substance; half an ounce in measure of each was then put into separate glasses, and one drachm in measure of the strong nitric acid was added.

The different infusions were then examined by solution of isinglass, and I found that those of galls, sumach, and oak wood, were not rendered turbid, whilst the contrary happened to the infusions of oak bark, and of the artificial substance; for these continued to precipitate gelatine, until four drachms or half an ounce of the nitric acid had been added to each half ounce of the infusion.

When the results of these experiments are compared, they seem to establish, that although the artificial product is by much the most indestructible of all the tanning substances, yet there is some difference in this respect even between the varieties of natural *tannin*; and that common catechu, and the *tannin* of oak bark, resist the effects of nitric acid much longer than galls, sumach, kascutti, and Pegu cutch. The last, as I have observed, is replete with mucilage, and by nitric acid yields a large quantity of oxalic acid; it also appears to be the most

destructible of all the varieties of catechu, and on this account I attempted, although without success, to promote the destruction of the properties of the artificial substance, by adding gum arabic in one case, and sugar in another, to different portions, previous to exposing it to the action of nitric acid. I am however, convinced, that the presence of gum or mucilage in natural substances which contain *tannin*, renders this more speedily destructible by nitric acid, and I shall soon have occasion to notice some experiments which tend to prove, that the presence of gum or mucilage in certain bodies, also prevents or impedes more or less the formation of the artificial tanning substance. The cause of this difference I am inclined to suspect is, that in those bodies the gum or mucilage is not simply mixed, but is present in a state of chemical combination, by which, certain modifications produced by the action of nitric acid upon the elementary principles of the original substance become facilitated.

§ III.

A. When sulphuric acid was added to a solution of the artificial tanning substance, the latter became turbid, and a copious brown precipitate subsided, which was soluble in boiling distilled water, and then was capable of precipitating gelatine.

B. The same effect was produced by muriatic acid; so that in these particulars, the artificial tanning substance was found to resemble precisely the *tannin* of galls and of other natural substances.*

C. Carbonate of potash, when added to a solution of the

* Mr. DAVY on the Constituent Parts of Astringent Vegetables. Phil. Trans. 1803, p. 240, 241.

artificial tanning substance, deepens the colour, after which, the solution becomes turbid and deposits a brown magma.

D. Five grains of the dry substance were dissolved in half an ounce of strong ammonia; the whole was then evaporated to dryness, and being dissolved in water, was found not to precipitate gelatine, unless a small portion of muriatic acid was previously added.

E. Another portion of the same substance which had been dissolved in ammonia was evaporated in a long necked matrass, and was kept in very hot sand during half an hour; at first some ammonia arose, and afterwards a yellow liquor which had the odour of burned horn. The residuum was then examined, and was found to be nearly insoluble in water, to which it only communicated a slight yellow tinge.

F. It is remarkable, that the dry artificial tanning substance, although prepared from vegetable matter, should, when placed on a hot iron, emit an odour very analogous to burned animal substances, such as horn, feathers, &c.; this I found also to be the case in the experiment which has been related, and I was desirous therefore to ascertain more accurately the effects of heat on this substance when distilled in close vessels.

I took some very pure vegetable charcoal which had been exposed to a red heat in a retort for more than an hour, and by nitric acid converted it into the artificial tanning substance.

Twenty grains of this, rendered as dry as possible, were put into a small glass retort, to which a proper apparatus terminating in a jar filled with quicksilver and inverted in a mercurial trough was adapted. The retort was placed in a small furnace, and was gradually heated by a charcoal fire until the bulb became red hot.

When the retort became warm, and after the expulsion of the atmospheric air, a very small portion of water arose, which settled like dew on the sides of the vessels; this was succeeded by a little nitric acid, from which the tanning substance had not been completely freed, and soon after a yellowish liquor came over, which was in so very small a quantity as only to stain the upper part of the neck of the retort: as nothing more seemed to be produced, I then raised the fire, when suddenly the vessels were filled with a white cloud, and so great a portion of gas was almost explosively produced, as to overset the jar; this gas, by its odour, appeared to be ammonia, which in the first instance had formed the white cloud, by combining with the vapour of the nitric acid with which the vessels were previously filled.* Another jar was speedily placed in the room of that which had been overturned, and a quantity of gas was slowly collected; this proved to be carbonic acid, excepting a very small part, which was not taken up by solution of caustic potash, and which as far as the smallness of the quantity would permit to be determined, appeared to be nitrogen gas. There remained in the retort a very bulky coal, which weighed eight grains and a half; this by incineration yielded one grain and a half of brownish white ashes, which consisted principally of lime, but whether any alkali was also present I cannot positively assert, as the trace which I thought I discovered of it was very slight.

I shall for the present postpone any remarks upon this experiment, as I wish to proceed in the account of others which have been made on the artificial tanning substance.

G. Fifty grains of this substance were dissolved in four

* After the experiment the receiver was found to be thinly coated with a white saline crust.

ounces of water, and were afterwards precipitated by dissolved isinglass, eighty-one grains of which became thus combined with forty-six grains of the tanning substance.

The remaining portion of the latter was not precipitated, and was therefore separated by a filter, and evaporated to dryness. It then appeared in the state of a light brittle substance of a pale cinnamon brown colour; and it is very singular, that although charcoal is an inodorous body, and although the artificial tanning substance, when properly prepared, is likewise devoid of smell, (unless a certain pungent sensation which may be perceived upon first opening a bottle containing the powder after agitation should be so termed, but which seems rather to be a mechanical effect) yet this substance possessed a strong odour not very unlike prepared oak bark, and this odour became much more perceptible when the substance was put into water, in which it immediately dissolved. The solution was extremely bitter, and acted but slightly on dissolved isinglass, with which, however, it formed some flocculi; with sulphate of iron it produced a brown precipitate; with muriate of tin one which was blackish brown; nitrate of lime had not any effect; but acetite of lead occasioned a very copious precipitate of a pale brown colour. This substance therefore appeared to be a portion of the tanning matter so modified, as partly to possess the characters of extract.*

Other experiments were made on the tanning substance prepared from various bodies, which by the dry and by the humid way had been previously reduced to the state of coal; but these I shall here omit, and shall pass to the description of a series of

* When added to a solution of carbonate of ammonia, it produced some effervescence, but its peculiar vegetable odour did not suffer any diminution.

experiments, by which I obtained a variety of the artificial tanning substance in a way different from that which has been related, and with which I was unacquainted when my former paper was written.

§ IV.

I made several unsuccessful attempts to form the artificial substance by means of oxymuriatic acid, and it therefore appeared certain, that although a variety of the tanning matter could be produced by the action of sulphuric acid on resinous substances, yet the most effective agent was nitric acid, which readily formed it when applied to any sort of coal.

But I nevertheless suspected, that possibly this substance, or something similar to it, might be produced without absolutely converting vegetable bodies into coal; for it seemed, as I have observed in my former paper, that this only served to separate the carbon in a great measure from the other elementary principles (excepting oxygen) which were combined with it in the original substance, and thus to expose it more completely to the effects of the nitric acid, as well as to prevent the formation of the various acid products, which are so constantly afforded by the organized substances when thus treated. At first I had some thoughts of employing touchwood in this experiment, but not being able immediately to procure any, it occurred to me, that indigo might probably answer the purpose; for from some experiments made by myself, as well as from those described by BERGMAN,* I well knew that the proportion of carbon in this substance is very considerable. The following experiment was therefore made.

* *Analysis Chemica Pigmenti Indici.* Opuscula Bergo. Tom. V. p. 36.

1. On one hundred grains of fine indigo which had been put into a long matrass, one ounce of nitric acid diluted with an equal quantity of water was poured, and, as the action of the acid was almost immediate and extremely violent, another ounce of water was added. When the effervescence had nearly subsided, the vessel was placed in a sand-bath during several days, until the whole of the liquid was evaporated.

On the residuum, which was of a deep orange colour, three ounces of boiling distilled water were poured, by which a considerable part was dissolved.

The colour of the solution was a most beautiful deep yellow, and the bitter flavour of it surpassed in intensity that of any substance in my recollection; it was examined by the following reagents.

Sulphate of iron produced a slight pale yellow precipitate.

Nitrate of lime only rendered it a little turbid, after which, a small portion of white powder subsided, which had the characters of oxalate of lime.

Muriate of tin produced a copious white precipitate, which afterwards changed to a yellowish-brown.

Acetite of lead formed a very beautiful deep lemon-coloured precipitate, which possibly may prove useful as a pigment.

Ammonia rendered the colour much deeper, after which the liquor became turbid, and a large quantity of fine yellow spiculated crystals was deposited, which being dissolved in water, did not precipitate lime from its solutions.

The flavour of these crystals was very bitter, and I suspect them to be composed of ammonia combined with the bitter principle first noticed by Welther.*

* Thomson's System of Chemistry, 2d edit. Vol. IV. p. 246.

Lastly, when dissolved isinglass was added to the yellow solution of indigo, it immediately became very turbid, and a bright yellow substance was gradually deposited, and coated the sides of the glass jar with a tough elastic film, which was insoluble in boiling water, and possessed the characters of gelatine combined with tanning matter.

By this experiment I therefore ascertained, that a variety of the artificial tanning substance could be formed without previously converting the vegetable body into coal; and I have since discovered, that although indigo more readily yields this substance than most of the other vegetable bodies, yet in fact, very few of these can be regarded as exceptions, when subjected to repeated digestion and distillation with nitric acid.

2.—A. In my former Paper I have stated, that common resin, when treated with nitric acid, yielded a pale yellow solution with water, which did not precipitate gelatine, and that it was requisite to develope part of the carbon in the state of coal by sulphuric acid, before any of the tanning substance could be produced; but having again made some of these experiments, I repeated the abstraction of nitric acid several times, and then observed, that the solution of resin in water acted upon gelatine similar to the solution of indigo which has been described, and formed a tough yellow precipitate, which was insoluble in boiling water.

With other reagents the effects were as follows.

Sulphate of iron, after 12 hours, formed a slight yellow precipitate.

Nitrate of lime did not produce any effect.

Muriate of tin, after 12 hours, afforded a pale brown precipitate.

And acetite of lead immediately formed a very abundant precipitate of a yellowish white colour.

I repeated this experiment on common resin, and remarked, that during each distillation nitrous gas was produced, whilst the strength of the acid which came over was diminished; the cause therefore of the change in the properties of the resin seemed to me very evident, and I was induced to extend the experiments to various resinous and other substances: but as the process was uniformly the same, I shall only notice the principal effects.

B. Stick lac, when separated from the twigs, and treated as above described, copiously precipitated gelatine.

C. Balsam of Peru during the process afforded some benzoic acid, and gelatine was precipitated by the aqueous solution.

D. Benzoin also, after the sublimation of some benzoic acid, yielded a residuum, which with water formed a pale yellow solution, of a very bitter flavour.

This solution with sulphate of iron afforded a slight pale yellow precipitate.

With nitrate of lime not any effect was produced.

The solution with muriate of tin became turbid, and a small quantity of brownish-white precipitate subsided.

Acetite of lead immediately produced a copious pale yellow precipitate.

And solution of isinglass formed a dense yellow precipitate, which was insoluble in boiling water.

E. Balsam of Tolu, like Balsam of Peru, and Benzoin, afforded some benzoic acid; and the residuum being dissolved in water, was found to precipitate gelatine.

F. As the results of the experiments on dragon's blood were

somewhat remarkable, I shall here more particularly relate them. One hundred grains of pure dragon's blood, reduced to powder, were digested in a long matrass with one ounce of strong nitric acid; the colour immediately changed to deep yellow, much nitrous gas was evolved, and to abate the effervescence, one ounce of water was added. The digestion was continued until a deep yellow dry mass remained, and the matrass being still kept in the sand-bath, a brilliant feather-like sublimate arose, which weighed rather more than six grains, and had the aspect, odour, and properties of benzoic acid.*

The residuum was of a brown colour, and with water formed a golden yellow-coloured solution, which by nitrate of lime was not affected.

With sulphate of iron it afforded a brownish-yellow precipitate.

With muriate of tin the result was similar.

With acetite of lead a lemon-coloured precipitate was produced.

Gold was precipitated by it in the metallic state, whilst the glass vessel acquired a tinge of purple.

And dissolved isinglass produced a deep yellow deposit, which was insoluble in boiling water.

A portion of the same dragon's blood was simply exposed to heat in the same matrass, but not any appearance of benzoic acid could be discovered. I am therefore induced to believe,

* According to these experiments, dragon's blood ought to be arranged with benzoin and the balsams, but as the samples of this drug are not always precisely similar, it would be proper to examine every variety. That which was employed in my experiments, was a porous mass of a dark red, and was sent to me by Messrs. ALLEN and HOWARD, of Plough Court, in Lombard Street.

that in the first experiment it was obtained as a product, and not as an educt, a fact which as yet has not been suspected.

G. Gum ammoniac afforded a brownish yellow solution, the flavour of which was very bitter and astringent.

By sulphate of iron, this solution only became of a darker colour, but did not form any precipitate.

Nitrate of lime rendered it turbid, and produced a slight precipitate.

Muriate of tin formed a copious yellow precipitate.

Acetite of lead produced a similar effect:

And gelatine yielded a bright yellow deposit, which was completely insoluble by boiling water.

H. Asa fœtida yielded a solution which also precipitated gelatine like the substances above described.

I. Solutions of elemi, tacamahac, olibanum, sandarach, copaiba, mastich, myrrh, gamboge, and caoutchouc, were next examined, but these, although they precipitated the metallic solutions, did not affect gelatine. It is possible, however, that they might have produced this effect, had they been subjected to a greater number of repetitions of the process.

K. Sarcocol, in its natural state, as well as the gum separated from it by water, when treated with nitric acid, did not precipitate gelatine; but produced effects on the metallic solutions similar to the above mentioned substances.

L. Gum arabic afforded oxalic acid, but not any of the tanning matter.

M. Tragacanth yielded an abundance of saclactic acid, of oxalic, and of malic acid, but not the smallest vestige of the artificial tanning substance.

N. Manna, when treated with nitric acid in the way above

described, afforded oxalic acid, part of which was sublimed in the neck of the vessel.

The residuum with water formed a brown solution, which yielded a pale yellow precipitate with sulphate of iron.

Muriate of tin produced a pale brown precipitate.

Acetite of lead formed one of a brownish-white hue.

Lime was copiously precipitated from the nitrate of lime in the state of oxalate; but not the smallest effect was produced on solution of isinglass.

O. Liquorice however afforded a different result; for, although the solution after the process with nitric acid resembled in appearance that which was yielded by manna, yet the effects were not the same.

Sulphate of iron, after twelve hours, produced a slight brown precipitate.

Muriate of tin had a similar effect.

Acetite of lead formed a brownish-red deposit.

Nitrate of lime also occasioned a brown precipitate.

And solution of isinglass rendered it very turbid, and produced a yellowish-brown precipitate, which was insoluble in boiling water, and possessed all the other characters of gelatine combined with the tanning substance.

P. Guaiacum, the properties of which are so singular in many respects, afforded results (when treated with nitric acid in the manner which has been described) different from the resins, although its external and general characters seem to indicate that it appertains to those bodies.

Nitric acid acted upon it with great vehemence, and speedily dissolved it. The residuum which was afterwards obtained, was also found to be almost totally soluble in water, and the

solution acted on the metallic salts like those which have already been noticed, but with gelatine it formed only a very slight precipitate, which was immediately dissolved by boiling water; and the remainder of the solution being evaporated, yielded a very large quantity of crystallized oxalic acid; so that in this respect guaiacum was found to resemble the gums, and to be totally unlike the resins.*

§ V.

As many vegetable substances when roasted, yield by decoction a liquid, which in appearance much resembles the artificial tanning matter when dissolved in water; I roasted some of the common dried peas, horse-beans, barley, and wheat flour, the decoctions of which however did not afford any precipitate by solution of isinglass.

Even the decoction of coffee did not yield any precipitate by this method, until several hours had elapsed, and I found that the precipitate so formed was permanently soluble in boiling water. But to explain this, we must recollect, it is extremely probable, that some peculiar nicety is required in the roasting of such bodies before the tanning substance can be developed; and this seems to be corroborated by some experiments which I made on the decoction of a sort of coffee prepared from the chicoreé (I suppose endive) root, which was given me by

* The properties of guaiacum which have been described, as well as those which were previously known, appear to indicate, that it is a peculiar substance of a nature distinct from the resins, balsams, and even the gum resins.

So remarkable indeed is this substance, that an accurate series of experiments on the whole of its properties may justly be placed amongst the chemical desiderata.

Sir JOSEPH BANKS; for although this decoction did not afford an immediate precipitate with solution of gelatine, and although the precipitate was also apparently dissolved by boiling water, yet upon cooling, the same precipitate was reproduced in its original state. I am therefore inclined to believe, that the tanning substance is really developed in many of the vegetable bodies by heat, but that a certain degree of temperature, not very easy to determine, is absolutely requisite for this purpose.

Before I conclude this section, it may be proper to observe, that when a small quantity of nitric acid was added to any of the above-mentioned decoctions, and when these had been subsequently evaporated to dryness, and afterwards dissolved in distilled water, they were converted into a tanning substance perfectly similar to that which is produced by the action of nitric acid on the varieties of coal.

§ VI.

In the preceding Paper, a variety of the tanning substance was slightly noticed, which was formed by the action of sulphuric acid upon common resin, elemi, amber, &c. &c. and as an instance has occurred of the formation of the same substance from camphor, accompanied by circumstances which tend to increase our knowledge of the properties of the latter, I shall here describe this experiment.

Experiment on Camphor with sulphuric Acid.

The effects produced on camphor by sulphuric acid have been but very superficially examined; for all that has hitherto been stated amounts to this, that camphor is dissolved by sulphuric acid, that a brown or reddish-brown solution is formed,

and that the camphor is precipitated unchanged from this solution by water. These facts, however, only relate to a certain period of the operation, for if this be long continued, other effects are produced, which I shall now describe.

A. To one hundred grains of pure camphor put into a small glass alembic, one ounce of concentrated sulphuric acid was added. The camphor immediately became yellow, and gradually dissolved, during which, the acid progressively changed to brownish-red, and afterwards to brown. At this period, scarcely any sulphureous acid was evolved, but in about one hour the liquid became blackish-brown; much sulphureous acid gas was then produced, and continued to increase during four hours, when the whole appeared like a thick black liquid, at which period not any odour or appearance of camphor could be perceived, but only that of the sulphureous acid. After two days, during which time the alembic had not been heated, there did not appear any alteration, unless that the production of sulphureous gas was much diminished. The alembic was then placed in a sand-bath moderately warm, by which, more of the sulphureous gas was obtained, but this also soon began to abate. After the lapse of two other days, I added gradually six ounces of cold water, by which the liquid was changed to reddish-brown, a considerable coagulum of the same colour subsided, the odour of sulphureous gas, which in some measure had still prevailed, was immediately annulled, and was succeeded by one which resembled a mixture of oils of lavender and peppermint.

The whole was then subjected to gradual distillation, during which, the water came over strongly impregnated with the odour abovementioned, accompanied by a yellowish oil

which floated on the top of it, and which, as far as could be ascertained, amounted to about three grains.

B. When the whole of the water was come over, there was again a slight production of sulphureous gas. I then added two ounces of water, which I drew off by distillation, but did not obtain any of the vegetable essential oil which has been mentioned, nor did the odour of it return, I therefore continued the distillation until a dry blackish brown mass remained; this was well washed with warm distilled water, by which, however, nothing was extracted; but when two ounces of alcohol were digested on it during twenty-four hours, a very dark brown tincture was formed.

The residuum was digested with two other ounces of alcohol in like manner, and the process was repeated until the alcohol ceased to act.

The residuum had now the appearance of a compact sort of coal in small fragments, it was then well dried, and after exposure to a low red heat in a close vessel weighed fifty-three grains.

C. The different portions of the solution formed by alcohol were added together, and being distilled by means of a water-bath, a blackish brown substance was obtained, which had the appearance of a resin or gum with a slight odour of caromel, and weighed 49 grains.

The products therefore which were thus obtained from 100 grains of camphor when treated with sulphuric acid, were,

	Grains.
A. An essential oil which had an odour somewhat resembling a mixture of lavender and peppermint, about -	3
B. A compact and very hard sort of coal in small fragments - - - - - -	53
C. And a blackish brown substance of a resinous appearance - - - - - -	49
	105

From this statement it appears, that there was an increase in the weight amounting to five grains, which I attribute partly to oxygen united to the carbon, and partly to a portion of water so intimately combined with the last product, that it could not be expelled from it by heat without subjecting it to decomposition. The properties of this substance were as follows:

1. It was extremely brittle, had somewhat of the odour of caromel, the flavour was astringent, and it speedily dissolved in cold water, and formed with it a permanent dark brown solution.

2. This solution yielded very dark brown precipitates by the addition of sulphate of iron, acetite of lead, muriate of tin, and nitrate of lime.

3. Gold was copiously precipitated by it from its solution in the metallic state; and

4. By solution of isinglass, the whole was completely precipitated, so that after three or four hours, a colourless water only remained.

The precipitate was nearly black, and was insoluble in boiling water; from which property, as well as from the effect produced upon prepared skin by the solution, it was evident, that the substance thus obtained from camphor, was a variety of

the artificial tanning matter, much resembling that which may be obtained from resinous bodies by means of sulphuric acid. But it must be observed, that this sort of tanning substance seems to act less powerfully on skin, than that which is prepared from carbonaceous substances by nitric acid, and the precipitate which the former produces with solution of gelatine is more flocculent and less tenacious, than that which in like manner is formed by the latter.

It is however remarkable, that when a small quantity of nitric acid was added to the solution of the substance obtained from camphor, and when after evaporating it to dryness, the residuum was dissolved in water, a reddish brown liquid was formed, which acted in every respect similar to the tanning substance obtained from the varieties of coal by nitric acid.

§ VII.

From the experiments which have been related, it appears, that three varieties of the artificial tanning substance may be formed, viz.

1st. That which is produced by the action of nitric acid upon any carbonaceous substance, whether vegetable, animal, or mineral.

2dly. That, which is formed by distilling nitric acid from common resin, indigo, dragon's blood, and various other substances; and,

3dly. That which is yielded to alcohol by common resin, elemi, asa fœtida, camphor, &c. after these bodies have been for some time previously digested with sulphuric acid.

Upon these three products I shall now make a few remarks,

which I have hitherto postponed, in order that the account of the experiments might not be interrupted.

The first variety is that which is the most easily formed; and from some experiments which were purposely made, I find that 100 grains of dry vegetable charcoal afford 120 of the tanning substance; but as it is extremely difficult completely to expel moisture, or even the whole of the nitric acid which has been employed,* an allowance of about three or four grains ought to be made, so that after this deduction we may conclude, that 100 grains of vegetable charcoal yield 116 or 117 of the dry tanning substance.

The proportions of the constituent parts of this substance I have not as yet ascertained; but from the manner by which it is produced, carbon is evidently the base of it, and is the predominating essential ingredient.

From § III. experiment F. it also appears, that the other component parts are oxygen, hydrogen, and nitrogen; for when the artificial tanning substance was distilled, ammonia and carbonic acid were obtained, exclusive of a very small portion of a yellow liquor, which stained the upper part of the retort, and which, from its tenacity and insolubility in water and alcohol, appeared to be of an oily nature.

As I had taken every precaution respecting the charcoal which had been employed, I was at first induced to consider the above facts as almost positively demonstrative of the presence of hydrogen in charcoal, but upon farther reflexion, and upon weighing some of the circumstances which attend the

* The most effectual method of expelling the nitric acid, is to reduce the tanning substance to powder, and repeatedly evaporate different portions of distilled water from it in a glass or porcelain basin.

formation of the artificial tanning substance, I still feel on this point very considerable doubt; for I have constantly observed, that diluted nitric acid, acts upon charcoal more effectually, in the formation of the tanning substance, than when it is employed in a concentrated state; and it appears therefore very probable, that hydrogen may have been afforded by a portion of water decomposed during the process. For admitting that the new compound (formed by the action of nitric acid upon charcoal) may possess a certain degree of affinity for hydrogen, this being exerted simultaneously with the affinity for oxygen possessed by nitrous gas, may (especially when the last is in a nascent state) effect a decomposition of a portion of water, the hydrogen of which would therefore enter into the composition of the tanning substance, whilst the oxygen would supply the place of part of that which had been taken from the nitric acid.

Many of the properties of the tanning substance prepared from coal by nitric acid are very remarkable, particularly those which have been noticed in § III. experiments F. and G.; for surely it is not a little singular, that this substance when burned should emit an odour so very similar to animal matter, notwithstanding that the tanning substance had been prepared from pure vegetable charcoal. And again in experiment G. the portion which had not been precipitated by solution of isinglass, was, when dried, found to possess a strong vegetable odour very analogous to oak bark, although charcoal is inodorous, and isinglass very nearly so.

But, after all, the most extraordinary properties of this substance are certainly those which so nearly approach it to the vegetable principle called *tannin;* for it perfectly resembles

this principle by its solubility in water and in alcohol, by its action upon gelatine and upon skin, by the effects which it produces upon metallic solutions, upon those of the earths, and of the alkalis.

The sulphuric and muriatic acids also affect the solutions of it as they do those of *tannin*; and the only marked difference which as yet has been found in the characters of the artificial substance and of *tannin*, is, that the former is produced, whilst the varieties of the latter are more or less destroyed by nitric acid. This, for the present at least, must draw a line of separation between them, but we must not forget, that even the varieties of *tannin* * do not accord in the degree of destructibility.

* I shall here venture to state some ideas which have occurred to me on the probable cause and mode of the formation of tannin.

Mr. Biggin has proved, that similar barks when taken from trees at different seasons, differ as to the quantities of tannin contained in them. (Phil. Trans. 1799, p. 259.)

Mr. Davy also observes, "that the proportions of the astringent principles in barks vary considerably according as their age and size are different."

"That in every astringent bark the interior white bark (which is the part next to the alburnum) contains the largest quantity of tannin. The proportion of extractive matter is generally greatest in the middle or coloured part; but the epidermis seldom furnishes either tannin or extractive matter."

Moreover Mr. Davy remarks "that the white cortical layers are comparatively most abundant in young trees, and hence their barks contain in the same weight a larger proportion of tannin than the barks of old trees." Phil. Trans. 1803, p. 264.

We find, therefore,

1st. That the proportion of tannin in the same trees is different at different seasons.

2dly. That tannin is principally contained in the white cortical layers, or interior white bark which is next to the alburnum or new wood: and

3dly. That these white cortical layers are comparatively most abundant in young trees, and that their barks consequently contain in the same weight more tannin than the barks of old trees.

I shall not make any remarks on the first of these facts, as it accords with other similar effects, which are the natural consequences of the processes and periods of vege-

The second variety of the tanning substance is obtained from a great number of vegetable bodies, such as indigo, dragon's blood, common resin, &c. &c. by digesting and distilling them with nitric acid. It is not, therefore, quite so readily prepared as that which was first described, and its relative quantity, when compared with that of the substance employed to produce it, is less.

As resin and some of the other bodies do not afford it until they have been repeatedly treated with nitric acid, and as during each operation nitrous gas is produced, whilst the strength of the acid which comes over is diminished, it seems almost

tation; but the second and third appear to be important; for they prove that tannin is principally formed, or at least deposited, in the interior white bark, which is next to the alburnum or new wood; so that in the very same part where the successive portions of new wood are to be elaborated and deposited, we find the principal portion of tannin.

It should seem, therefore, that there is an intimate connexion between the formation of new wood and the formation of tannin in such vegetables as afford the latter; and this idea is corroborated when the chemical nature of these substances is considered.

From experiments made on the ligneous substance of vegetables, or the woody fibre, it appears to be composed of carbon, oxygen, hydrogen, and nitrogen, but of these its principal and essential ingredient is carbon.

In like manner carbon is unquestionably the basis and principal ingredient of tannin. Considering, therefore, that both of these substances consist principally of carbon, that tannin is secreted in that part of barks where the formation and deposition of new wood take place, and that the quantity of tannin is the most considerable in young trees, and seems therefore to keep pace with their more vigorous growth and consequent rapid formation of wood, it appears very probable that those vegetables which contain tannin, have the faculty of absorbing more carbon and of the other principles than are immediately required in the formation of the different proximate vegetable substances, especially the woody fibre: that this excess, by chemical combination, becomes tannin, which is secreted in the white interior bark: that in this state it is a principle peculiarly fitted to concur by assimilation to form new wood: that it is therefore subsequently decomposed at the proper period, and is employed in the

evident, that this tanning substance is formed in consequence of part of the oxygen of the nitric acid becoming combined with the hydrogen of the original body, so as to form water; and the carbon being thus in some measure denuded, is rendered capable of being gradually acted upon by the nitric acid in a manner nearly similar to that, which takes place when it has been previously converted into coal.

The colour of the precipitates which this tanning substance yields with gelatine is constantly pale or deep yellow, whilst that of the precipitates formed by the first variety is always brown; I am therefore induced to believe, that the different colours of the precipitates produced by the varieties of tannin depend on the state of their carbon.

When resin and the other bodies were treated as above described with nitric acid, the quantity obtained of the tanning substance was much less than when an equal quantity of coal was employed, or even when these bodies had been previously converted into coal in the humid way by sulphuric acid.

The cause of this seems to be, that a number of other products are simultaneously formed, all of which require more or less of carbon as a constituent ingredient, so that, in consequence of the affinities which prevail under the existing circumstances,

formation of the new wood: that there is not a continual accumulation of tannin in the vegetables which afford it, as it is successively formed in and with the white cortical layers, and is successively decomposed by concurring to form new wood: and, lastly, that as the vegetable approaches more nearly to the full maturity of its growth, when wood is less rapidly and less plentifully formed, so in like manner less tannin is secreted, for the fabric being nearly completed, fewer materials are required.

Such I am inclined to suspect, from the facts which have been adduced, to be the cause and mode by which tannin is formed in oaks and other vegetables, but I make this statement only as a probable conjecture, which may be refuted or confirmed by future observations.

some bodies by treatment with nitric acid afford but little, and others none of the tanning substance.

The greatest proportion of this substance was yielded by indigo, common resin, and stick lac.

The quantity obtained from asa fœtida and gum ammoniac was less.

Benzoin, balsam of Tolu, balsam of Peru, and dragon's blood, were inferior to the former in this respect, so that the developement or rather production of benzoic acid * appeared partly to counteract the formation of the tanning substance.

* The expression "*production of benzoic acid*" may appear objectionable, and I shall therefore take this opportunity to observe, that I much suspect the present established opinion respecting the balsams and benzoic acid to be erroneous. For the balsams are defined as bodies composed of resin and benzoic acid; consequently the latter, when obtained in a separate state, is considered as an original ingredient or educt.

I am however inclined to a contrary opinion, for I consider the balsams as peculiar substances, which, although nearly approaching to the nature of resins, are nevertheless different in respect to the original combination of their elementary principles, which combination however is with much facility modified by various causes, and especially by a certain increase of temperature, so that a new arrangement of the elementary principles takes place, part being formed into resin, and part into benzoic acid.

Many facts appear more or less to support this opinion; for whether benzoic acid is obtained by simple sublimation, or by merely digesting benzoin in boiling water, according to GEOFFROY's method, or by the addition of lime, as recommended by SCHEELE, or by employing alkalis in a similar manner, nothing positive can be inferred from any of these operations to prove that benzoic acid is obtained as an educt, but rather the contrary, when we reflect on the affinities which are most likely to prevail under the circumstances of the different processes, and on the variable proportions of the benzoic acid; and although benzoic acid has been discovered in the urine of infants, in that of many adults, and constantly in that of graminivorous quadrupeds, such as the camel, the horse, and the cow, (*Système des Connoissances Chimiques*, par FOURCROY, 4to edit. Tome IV. p. 158;) yet all this certainly appears to be in favour of its being a chemical product.

I have observed, when benzoin, balsam of Tolu, and balsam of Peru, were dissolved

but oxalic acid when formed in any considerable quantity, seemed *absolutely to prevent* the formation of this substance; for whilst abundance of the former was obtained from gum arabic, tragacanth, manna, and guaiacum, not any of the latter could be produced.

Common liquorice appears at first to be an exception, but from the smallness of the quantity and the colour of the precipitate which it produced with solution of isinglass, I am almost convinced that the tanning substance was formed by the action of the nitric acid on a portion of uncombined carbon,

in sulphuric acid, that a great quantity of beautifully crystallized white benzoic acid was sublimed during digestion; and as it is produced in so very pure a state by this single and simple operation, I would recommend a trial of the process to those who prepare benzoic acid for commerce; but I am not certain whether this mode may prove more economical than those which at present are employed.

When dragon's blood, however, was treated in the same manner with sulphuric acid, I could not obtain a particle of benzoic acid; nor did I succeed much better when I had recourse to lime, according to Scheele's process; for although a considerable quantity of the substance was thus rendered soluble in water, yet by the addition of muriatic acid I obtained only a slight appearance of benzoic acid accompanied by a copious precipitate of red resin, notwithstanding that the solution had acquired a powerful and peculiar balsamic odour.

But in a former part of this Paper I have stated, that when dragon's blood was dissolved in nitric acid, and afterwards evaporated to dryness, it yielded about *6 per cent.* of benzoic acid. Now if this had been originally present in dragon's blood in the state of benzoic acid, some stronger evidence of it might reasonably have been expected in each process, but this not being the case, I am inclined to consider it as produced, and not educed, by the action of the nitric acid on the original principles of the dragon's blood; and I am also persuaded that similar but more general effects take place when benzoin or any of the balsams are subjected to the different processes by which benzoic acid is obtained; so that to me, this last seems to be as much a chemical product, as the oxalic, the acetous, and other of the vegetable acids.

The succinic acid also appears to be a product and not an original ingredient of amber.

which being in a state approaching to coal, is probably the cause of the blackness of the common liquorice.

As the formation of the tanning substance has been my principal object, I have not thought it necessary to enter at present into too minute a detail of other particulars, and have therefore only thus cursorily noticed some of the principal effects produced by nitric acid on the resins, balsams, &c. Those however who are conversant with chemistry, will undoubtedly perceive that these effects deserve to be accurately investigated, and that the resins, balsams, gum resins, and gums, should be regularly examined by every possible method, not merely on account of the individual substances which may become the subjects of experiment, but because there is reason to expect that from such an investigation, medicine, with the arts, and manufactures, may derive many advantages, whilst the mysterious processes and effects of vegetation may very probably receive considerable elucidation.

Concerning the third variety of the tanning substance, which is produced by the action of sulphuric acid on the resins, gum resins, &c. I shall here add but little to that which I have already stated in the latter part of the second section of my first paper, and in the account which I have lately given of an experiment on camphor.

This variety appears to be uniformly produced during a certain period of the process, but by a long continuance of the digestion, I have reason to believe that it is destroyed.

Substances, such as the gums, which afford much oxalic acid by treatment with other acids, do not apparently yield any of this tanning substance.

The energy of its action on gelatine and skin is certainly

inferior to that of the first variety, into which however (as we have seen) it may easily be converted by nitric acid.

From the mode of its formation, there does not appear to be any positive evidence that it contains nitrogen like the first and second varieties, and perhaps the absence of nitrogen may be the cause of its less powerful action; this I have not as yet ascertained, but it is my intention more particularly to notice in a future Paper the general properties of this substance.

XXII. *On the Discovery of Palladium; with Observations on other Substances found with Platina.* By William Hyde Wollaston, M. D. Sec. R. S.

Read June 4, 1803.

HAVING some time since purified a large quantity of platina by precipitation, I have had an opportunity of observing various circumstances in the solution of this singular mineral, that have not been noticed by others, and which, I think, cannot fail to be interesting to this Society.

As I have already given an account of one product obtained from that ore, which I considered as a new metallic substance, and denominated Rhodium, I shall on the present occasion confine myself principally to those processes by which I originally detected, and subsequently obtained another metal, to which I gave the name of *Palladium*, from the planet that had been discovered nearly at the same time by Dr. OLBERS.

In the course of my inquiries I have also examined the many impurities that are usually mixed with the grains of platina, but I shall not think it necessary to describe minutely substances which have already been fully examined by others.

§ I. *Ore of Iridium.*

I must however notice one ore, that I find accompanies the ore of platina, but has passed unobserved from its great resemblance to the grains of platina, and on that account is

scarcely to be distinguished or separated from them, excepting by solution of the platina; for the grains of which I speak are wholly insoluble in nitro-muriatic acid. When tried by the file, they are harder than the grains of platina; under the hammer they are not in the least degree malleable; and in the fracture they appear to consist of lamina possessing a peculiar lustre; so that although the greater number of them cannot, as I have before observed, be distinguished from the grains of platina, the laminated structure sometimes occasions an external form by which they may be detected. With a view to be absolutely certain that there exist grains in a natural state, which have not been detached by solution from the substance of the grains of platina, I have separated from the mixed ore as many as enabled me to ascertain their general composition.

Their most remarkable quality is their great specific gravity, which I have found to be as much as 19.5, while that of the crude grains of platina has not, in any experiment that I have made, exceeded 17.7. From this circumstance it might naturally be conjectured that they contain a greater quantity of platina than the grains in general: by analysis, however, they do not appear to me to contain the smallest quantity of that metal, but to be an ore consisting entirely of the metals that were found by Mr. TENNANT in the black powder which is extricated by solution from the grains of platina, and which he has called Iridium and Osmium. But, since the specific gravity of these grains so much exceeds that of the powder, which by my experiments has appeared to be, at the utmost, 14.8, I have thought it might deserve inquiry whether their chemical composition is in any respect different. For this purpose I have selected a portion of them, and have requested Mr. TENNANT

to undertake a comparative examination, from whose well known skill in chemical inquiries, as well as peculiar knowledge of the subject, we have every reason to expect a complete analysis of this ore.

§ II. *Hyacinths.*

Among those bodies which may be separated from the ore of platina, in consequence of their less specific gravity, by a current of water or of air, there may be discerned a small proportion of red crystals so minute, that 100 of the largest I could collect weighed scarcely $\frac{1}{10}$ of a grain. The quantity which I possess is consequently too small for chemical analysis; but their physical properties are such as correspond in every respect with those of the hyacinth. I was first led to compare them with that stone by their specific gravity, which I conjectured to be considerable from their accompanying other substances, that appear to have been collected together solely by reason of their superior weight.

Like the hyacinth, these crystals lose their colour immediately and entirely when heated; they also agree with it in their hardness, which is barely sufficient to scratch quartz, but is decidedly inferior to that of the topaz.

The principal varieties of their form may be very well understood by description.

1st. In its most simple state the crystal may be considered as a rectangular prism terminated by a quadrilateral obtuse pyramid, the sides of which sometimes arise direct from the sides of the prism; but,

2dly. The position of the pyramid is generally such that its sides arise from the angles of the prism. In this case the sides of the prism are hexagons.

3dly. It is more usual for the prism to have eight sides by truncation of each of its angles, and at each extremity eight additional surfaces occupying the place of the eight linear angles between the prism and terminating pyramid of the 2d variety. The complete crystal has then thirty-two sides.

4thly. The eight surfaces last mentioned, as interposed between the prism and pyramid, are sometimes elongated into a complete acute pyramid having eight sides arising from the angles of an octahedral prism.

The 3d form above described, corresponds so entirely with that given by the Abbé Haüy * as one of the forms of the hyacinth or jargon, that I have little reason to regret my inability to obtain chemical evidence of the composition of these crystals.

Those, and other impurities, I usually separated, as far as was practicable, by mechanical means, previously to forming the solution of platina, which has been the principal object of my attention.

§ III. *Precipitation of Platina.*

When a considerable quantity of the ore had been dissolved, and I had obtained, in the form of a yellow triple salt, as much of the platina as could be precipitated by sal ammoniac, clean bars of iron were next immersed in the solution for the purpose of precipitating the remainder of the platina.

For distinction it will be convenient to call this, which in fact consists of various metals, the first metallic precipitate.

The treatment of this precipitate differed in no respect from that of the original ore. It was dissolved as before, and a portion

* *Traité de Mineralogie*, Pl. XLI. fig. 17.—*Journ. des Mines*, No. 26, fig. 9.

of platina precipitated by sal ammoniac; but it was observable that the precipitate now obtained was not of so pale a yellow as the preceding. Nevertheless the impurity was in so small quantity, that the platina reduced from it by heat did not differ discernibly from that obtained from the purest yellow precipitate.

At this time I found it advantageous to neutralize the solution with soda, and to employ a solution of green sulphate of iron for the precipitation of the gold, of which, I believe, a portion may always be obtained from the mixed ore; but I have observed in experiments upon any quantities of mere grains of crude platina carefully selected, that the smallest portion of gold could not be detected as a constituent part of the ore itself.

Bars of iron were subsequently employed as before for recovering the platina that remained dissolved, together with those substances which I have since found to accompany it.

The precipitate thus obtained, which I distinguish by the name of the second metallic precipitate, was to appearance of a blacker colour than the former, and was a finer powder.

As I was not at first prepared to expect any new bodies, I proceeded to treat the second precipitate, as the former, by solution and precipitation. But I soon observed appearances which I could not explain by supposition of the presence of any known bodies, and was led to form conjectures of future discoveries, which subsequent inquiry has fully confirmed.

When I attempted to dissolve this second metallic precipitate in nitro-muriatic acid, I was surprised to find that a part of it resisted the action of that solvent, notwithstanding any variations in the relative proportions or strength of the acids

employed to form the compound, and although the whole of this powder had certainly been twice completely dissolved.

The solution formed in this case was of a peculiarly dark colour, and when I endeavoured to precipitate the platina from it by sal ammoniac, the precipitate obtained was small in quantity, and, instead of being yellow, was of a deep red colour, arising from an impurity which I did not at that time understand, but which we since know, from the experiments of Mr. DESCOTILS, is occasioned by the metal now called iridium.

The solution, instead of being rendered pale by the precipitation of the platina, retained its dark colour in consequence of the other metals that remained in solution; but, as I had not then learned the means of separating them from each other, and as the quantity of fluid which accumulated occasioned me some inconvenience, I decomposed it by iron, as in the former instances, and formed a third metallic precipitate, which could more commodiously be reserved for subsequent examination.

In this last step I committed an error which afterwards occasioned me considerable difficulty, for I found that a great part of this precipitate consisting of rhodium was unexpectedly rendered insoluble by this treatment, and resembled the residuum of the second metallic precipitate abovementioned.

As I have already communicated to this society, in my Paper upon rhodium, the process by which I subsequently avoided this difficulty, I shall at present return to a previous stage of my progress, and relate the means by which I first obtained palladium in my attempts to analyze the second metallic precipitate.

§ IV. *Separation of Palladium.*

There was no difficulty in ascertaining the presence of lead as one of the ingredients of this precipitate, by means of muriatic acid, which dissolved lead and iron and a small quantity of copper. It was equally easy to obtain a larger portion of copper by dilute nitrous acid, with which it formed as usual a blue solution. But when I endeavoured to extract the whole of the copper by a stronger acid, it was evident, from the dark brown colour of the solution, that some other metallic ingredient had also been dissolved. I at first ascribed this colour to iron; but, when I considered that this substance had been more slowly acted upon than copper, I relinquished that hypothesis, and endeavouring to precipitate a portion of it by a clean plate of copper, I obtained a black powder adhering to a surface of platina on which I had placed the solution. As this precipitate was soluble in nitric acid, it evidently consisted neither of gold nor platina; as the solution in that acid was of a red colour, the metal could not be either silver or mercury; and as the precipitation of it by copper excluded the supposition of all other known metals, I had reason to suspect the presence of some new body, but was not fully satisfied of its existence until I attempted the precipitation of it by mercury.

For this purpose I agitated a small quantity of mercury in the nitrous solution previously warmed, and observed the mercury to acquire the consistence of an amalgam. After this amalgam had been exposed to a red heat, there remained a white metal, which could not be fused before the blow pipe. It gave a red solution as before in nitrous acid; it was not

precipitated by sal ammoniac, or by nitre; but by prussiate of potash it gave a yellow or orange precipitate; and in the order of its affinities it was precipitated by mercury but not by silver.

These are the properties by which I originally distinguished palladium; and by the assistance of these properties I obtained a sufficient quantity for investigating its nature more fully.

There were, however, various reasons which induced me to relinquish the original process of solution in nitrous acid and precipitation by mercury; for although I found the metal thus obtained to be nearly pure, the necessity of agitating the solution with the mercury was very tedious, and the waste was also considerable; for in the first place it seemed that nitrous acid would not extract all the palladium from any quantity of the second metallic precipitate, neither would mercury reduce the whole of what was so dissolved. I therefore substituted a process dependent on another of its properties. I had observed that this metal differed from platina in not being precipitated from nitro-muriatic acid by nitre or by other salts containing potash; for although a triple salt is thus formed, this salt is extremely soluble, while that of platina on the contrary requires a large quantity of water for its solution. On that account a compound *menstruum* consisting of nitrate of potash dissolved in muriatic acid is unfit for the solution of platina, but dissolves palladium nearly as well as common nitro-muriatic acid in which there is no potash present.*

In five ounces of muriatic acid diluted with an equal quantity of water, I dissolved one ounce of nitre, and formed a solvent

* I have found that gold may also be dissolved with equal facility by the same solvent, and nearly in the same proportion. Ten grains of nitre added to a proper quantity of muriatic acid are sufficient for sixteen grains of either gold or palladium.

for palladium that possesses little power of acting on platina, so that by digesting any quantity of the second metallic precipitate till there appeared to be no farther action, I procured a solution from which by due evaporation were formed crystals of a triple salt, consisting of palladium combined with muriatic acid and potash. These are the crystals which I have on a former occasion * mentioned as exhibiting a very singular contrast of colours, being bright green when seen transversely, but red in the direction of their axis; the general aspect, however, of large crystals is dark brown.

From the salt thus formed and purified by a second crystallization, the metal may be precipitated nearly pure by iron or by zinc, or it may be rendered so by subsequent digestion in muriatic acid.

§ V. *Reasons for thinking Palladium a simple Metal.*

From the consideration of this salt alone I thought it highly probable that the substance combined in it with muriate of potash was a simple metal, for I know of *no instance in chemistry of a distinctly crystallized salt containing more than two bases combined with one acid.* I nevertheless endeavoured by a suitable course of experiments to obviate all probable objections. After examining by what acids it might be dissolved and by what reagents it might be precipitated, I combined it with various metals, with platina, with gold, with silver, with copper, and with lead; and when I had recovered it from its alloys so formed, I ascertained that, after every mode of trial it still retained its characteristic properties, being soluble in nitrous acid, and precipitable from thence by mercury, by green

* Phil. Trans. 1804, p. 428.

sulphate of iron, by muriate of tin, by prussiate of potash, by each of the pure alkalis, and by hydrosulphurets.

The precipitate obtained in each case was also found to be reducible by mere heat to a white metal, that, except in very small quantities, could not be fused alone by the blowpipe, but could very readily be fused with sulphur, with arsenic, or with phosphorus, and in all other respects resembled the original metal.

The only hypothesis, on which I thought it possible that I could be deceived, arose from the recollection of the error, which subsisted for a few years, respecting the compound formerly called siderite. It was possible that some metallic or other fixed acid might unite too intimately with either a known or an unknown metal to be separated by the more common simple affinities. I consequently made such attempts as appeared best calculated to disunite a compound so constituted.

Having boiled the oxide with pure alkalis, and found it to be unaltered, I thought the affinities of lime or lead might be more likely to detect the presence of the phosphoric or of any known metallic acid; and accordingly I made various attempts by muriate and nitrate of lime, as well as by nitrate of lead, to effect a decomposition of the supposed compound. In the experiment on which I placed the greatest reliance, I poured liquid muriate of lime into a solution of palladium in nitro-muriatic acid, and evaporated the mixture to dryness, intending thereby to expel any excess of acid that might have been left in the solution, and to render either phosphate of lime, or any compound of lime with a metallic acid, insoluble in water. The residuum however was very readily dissolved by water, and

consisted merely of muriate of lime and muriate of palladium, without any appearance of decomposition.

When I found all my endeavours directed to that end wholly unsuccessful, I no longer entertained any doubt of this substance being a new simple metal, and accordingly published a concise delineation of its character; but by not directing the attention of chemists to the substance from which it had been extracted, I reserved to myself an opportunity of examining more at leisure many anomalous phenomena, that had occurred to me in the analysis of platina, which I was at a loss to explain, until I had learned to distinguish those peculiarities, that I afterwards found to arise from the presence of rhodium.

§ VI. *Additional Properties of Palladium.*

In my former Paper on that subject I also added some observations upon the properties and origin of palladium, describing only such a mode of obtaining it from platina as should avoid the introduction of any unnecessary ingredient which might possibly be misinterpreted, and omitted one of the most distinguishing properties of palladium, by means of which it may be obtained with the utmost facility by any one who possesses a sufficient quantity of the ore of platina.

To a solution of crude platina, whether rendered neutral by evaporation of redundant acid, or saturated by addition of potash, of soda, or ammonia, by lime or magnesia, by mercury, by copper, or by iron, and also whether the platina has or has not been precipitated from the solution by sal ammoniac, it is merely necessary to add a solution of prussiate of mercury, for the precipitation of the palladium. Generally for a few seconds,

and sometimes for a few minutes, there will be no appearance of any precipitate; but in a short time the whole solution becomes slightly turbid, and a flocculent precipitate is gradually formed, of a pale yellowish-white colour. This precipitate consists wholly of prussiate of palladium, and when heated will be found to yield that metal in a pure state, amounting to about 4 or 5 tenths *per cent.* upon the quantity of ore dissolved.

The prussiate of mercury is peculiarly adapted to the precipitation of palladium, exclusive of all other metals, on account of the great affinity of mercury for the prussic acid, which in this case prevents the precipitation of iron or copper; but the proportion of mercury does not by any means influence the quantity of palladium, for I have in vain endeavoured, in the above experiment on crude platina, to obtain a larger quantity of palladium than I have stated by using more of the prussiate of mercury, or to procure any precipitate by the same means from a solution of pure platina.

The decomposition of muriate of palladium by prussiate of mercury is not effected solely by the superior affinity of mercury for the muriatic acid, but is assisted also by the greater affinity of prussic acid for palladium; for I have found that prussiate of palladium may be formed by boiling a precipitated oxide of palladium in a solution of prussiate of mercury.

The prussiate of mercury is consequently a test by which the presence of palladium may be detected in any of its solutions; but it may be worth observing, that the precipitate obtained has not in all cases the same properties. In general, this compound is affected by heat similarly to other prussiates, but when the palladium has been dissolved in nitrous acid and

precipitated from a neutral solution by prussiate of mercury, the precipitate thus formed has the property of detonating when heated. The noise is similar to that occasioned by firing an equal quantity of gunpowder, and accordingly the explosion is attended with no marks of violence unless occasioned by close confinement. The heat requisite for this purpose is barely sufficient to melt bismuth, consequently is about 500° of FAHRENHEIT. The light produced is proportionally feeble, and can only be seen in the absence of all other light.

In endeavouring to dissolve a piece of palladium in strong colourless nitric acid for the purpose of forming the detonating prussiate, I found that, although the acid shortly acquired a red colour surrounding the metal, the action of the acid was extremely slow, and I was surprised to observe a fact that appears to me wholly singular: the metal was taken up without any extrication of nitrous gas; and this seemed to be the cause of the slow solution of this metal, as there was not that circulation of the fluid, which takes place in the solution of other metals until the acid is nearly saturated.

As the want of production of gas appeared to retard the solution of palladium, I tried the effect of impregnating a quantity of the same acid previously with nitrous gas, and observed its action to be very considerably augmented, although the experiment was necessarily tried in the cold, because the gas would have been expelled by the application of heat.

Beside those properties which are peculiar to palladium there are others, not less remarkable, which it possesses in common with platina. I have on a former occasion mentioned that these metals resemble each other in destroying the colour of a large quantity of gold. Their resemblance, however, in other

properties is not less remarkable, more especially in the little power they possess of conducting heat, and in the small degree of expansion to which they are liable when heated.

For the purpose of making a comparison of the conducting powers of different metals, I endeavoured to employ them in such a manner, that the same weight of each metal might expose the same extent of surface. With that view I selected pieces of silver, of copper, of palladium, and platina, which had been laminated so thin as to weigh each 10 grains to the square inch. Of these I cut slips $\frac{4}{10}$ of an inch in breadth, and four inches long; and having covered their surfaces with wax, I heated one extremity so as to be visibly red, and, observing the distance to which the wax was melted, I found that upon the silver it had melted as far as $3\frac{1}{4}$ inches: upon the copper $2\frac{1}{2}$ inches: but upon the palladium and upon the platina only 1 inch each: a difference sufficient to establish the peculiarity of these metals, although the conducting power cannot be said to be simply in proportion to those distances.

In order to form some estimate of the comparative rate of expansion of these metals, I rivetted together two thin plates of platina and of palladium; and observing that the compound plate, when heated, became concave on the side of the platina, I ascertained that the expansion of palladium is in some degree the greater of the two. By a similar mode of comparison I found that palladium expands considerably less than steel by heat; so that if the expansion of platina between the temperatures of freezing and boiling water be estimated at 9 parts in 10,000, while that of steel is known to be about 12, the expansion of palladium will probably not be much more or less than 10, or one part in 1000 by the same difference of temperature.

It must, however, be acknowledged that the method I have pursued is by no means sufficient for determining the precise quantity of expansion of any substance; but I have not been induced to bestow much time on such an inquiry, since the extreme scarcity of palladium precludes all chance of any practical utility to be derived from a more accurate investigation.

XXIII. *Experiments on a Mineral Substance formerly supposed to be Zeolite; with some Remarks on two Species of Uran-glimmer.* By the Rev. William Gregor. *Communicated by* Charles Hatchett, *Esq. F. R. S.*

Read July 4, 1805.

THIS mineral is raised in a mine called Stenna Gwyn, in the parish of St. Stephen's, in Branwell, in the county of Cornwall; the principal production of which is the compound sulphuret of tin, copper, and iron.

Description.

Two species of this mineral are found, assuming a marked difference in external character.

The first and most common one consists of an assemblage of minute crystals, which are attached to quartz crystals, in tufts, which diverge from the point of adherence, as from a centre. These tufts vary, as to the number of crystals, of which they are composed, and are light and delicate in the forms which they assume, or they are grouped together according to a variety of degrees of proximity and compactness. Sometimes they fill the whole cavity of a stone, with little or no interruption; in other specimens they are seen partially spreading over the sides and pointed pyramids of quartz crystals.

In some cases these grouped tufts adhere very pertinaciously to the stone which bears them; in others, they are easily separable, in comparatively large pieces, from the quartz, he

impressed form of which the pieces thus separated, retain. The surface of these, which was in immediate contact with the quartz, exhibits the several minute crystals of which the mass consists, matted together in various directions.

These crystalline assemblages are, in general, white; a nearer inspection of the individual crystals proves that they are transparent. Sometimes they are stained of a yellowish hue by ochry water.

The size of these crystals varies considerably in different specimens. Sometimes they assume the appearance of a white powder raised up in small heaps, upon the surface of the stone, to which they adhere. In other specimens they resemble a tender down. And the larger sort varies, in relative size, in the proportion, perhaps, in which a human hair, horse-hair, and a hog's bristle, severally differ from each other in magnitude. They seldom exceed a quarter of an inch in length. The figure of these crystals is not easily ascertainable, on account of their minuteness. By the help of a very powerful microscope, they appear to consist of four-sided prisms; where these are broken off, the section exhibits a rhomboidal, approaching indeed to an elliptical figure, from the circumstance of the angles of the prism being worn away; but that the prism itself is rhomboidal, cannot be inferred from hence, unless we could be certified, that the section were at right angles with the axis of it.

Imbedded amongst these crystals two species of crystalline laminæ are frequently discoverable: the one consisting of parallelopipedon plates with truncated angles, applied to each other, of a green colour of various tints, from the emerald to the apple-green: the other species, consisting of an assemblage

of square plates, which vary in thickness. The angles of the several square laminæ, which are applied to each other are not always coincident. They are of a bright wax yellow. The sides of the largest of these square laminæ is about a quarter of an inch. This last species is frequently found adhering to the sides of quartz crystals, in the cavities of granite.

The other species of this mineral consists of an assemblage of crystals closely compacted together in the form of mammillary protuberances, in general, of the size of small peas, intimately connected with each other. A stratum of these about $\frac{1}{4}$ of an inch thick is spread upon a layer of quartz, in the cavities or fissures of a species of compact granite. The striæ of which these mamillæ consist diverge from a centre, like zeolite. Some of the individual striæ, in some cases, overtop their fellows, in these globular assemblages, and evidently assume, on their projecting points, a crystallized form.

A.

(1.) The detached crystals of the former species are easily reduced to powder, of a brilliant whiteness. At the temperature 56° of FAHRENHEIT, its specific gravity was found to be 2,22.

(2.) The hardness of the more compact species is sufficient to scratch calcareous spar. At the temperature 55°, its specific gravity was 2,253. It does not imbibe water.

(3.) Some of the crystals exposed, on charcoal, to the flame of the blowpipe suddenly and strongly driven upon them, decrepitate: if they are gradually exposed to the flame they grow opaque, and become more light and tender: but they show no signs of fusion under the strongest heat.

(4.) The phosphate of soda and ammonia takes up a piece

of this mineral without effervescence, but it swims about the fused globule, unaltered. Borax dissolves a fragment of a crystal, and the globule remains transparent.

(5.) Some of this mineral, reduced to a fine powder, was mixed with about half its weight of pounded quartz, and kneaded with water into a ball: but as soon as the mass became dry, all cohesion was destroyed, and it fell into powder.

(6.) Sulphuric acid, poured upon some of it, caused no effervescence, nor was there any perceptible vapour extricated.

(7.) Some of the pulverized crystals were put into a crucible of platina, and sulphuric acid was poured upon them. The crucible was covered with a piece of glass, and placed in warm sand. On examination of the crucible and its contents, after some time, it appeared that the greater part of the mineral had been dissolved, but the surface of the glass cover was not in the least affected.

(8.) Some of the crystals were introduced into a small glass retort, to which a receiver was adapted. The retort was exposed to the heat of a charcoal fire. A fluid distilled over into the receiver, which had a peculiar empyreumatic smell. It changed litmus-paper to a faint red. It produced no change in a solution of nitrate of silver; but it caused a white precipitate in a solution of nitrate of mercury. I attributed these phænomena, at the time, to a small bit of the feather with which I had swept the powder into the retort, and which, I thought, had fallen into it. A slight whitish crust was also produced in the neck of the retort, but the smallness of the quantity did not admit of examination.

(9.) Some of this mineral exposed to a red heat, for about ten minutes, lost in weight at the rate of 25½ *per cent.* Another

portion, exposed to a stronger heat for more than an hour, lost 30¼ *per cent.* This operation was performed in a crucible of platina; the cover of which gave some indications as if a slight portion of the finer parts had been volatilized.

Some of the compact species, after exposure to a red heat for one hour, experienced a diminution in weight of 30 *per cent.*

(10.) The sulphuric, muriatic and nitric acids, aided by a long digesting heat, effect nearly a complete solution of this substance. The quantity of the undissolved residuum is diminished in proportion to the purity of the mineral employed.

(11.) The nitrate of silver, as well as the muriate of barytes, produce no change in the solution of this substance in nitric acid.

(12.) The solutions of this substance in muriatic and nitric acids, cannot be brought to crystallize.

B.

(1.) I selected some of the crystals of this substance, as free as it was possible from extraneous matter. 50 grains grossly pounded were exposed, in a platina crucible, to a red heat for one hour. They weighed, *whilst still warm*, 35¼ grains, which is a loss of 28¼ *per cent.* 25 grains of the same parcel, from which I had taken the former, exposed to a heat of longer continuance and greater intensity, were diminished in weight, at the rate of 30¼ *per cent.*

(2.) The powder still preserved its pure whiteness. It was transferred into a matrass, and nitric acid poured upon it, which soon began to act upon it. The matrass was placed, for many hours, in a digesting heat. A solution of the whole

of the substance, except a small portion, was effected. I added a few drops of muriatic acid, and continued the digestion.

(3.) The acid was now diluted with distilled water, and poured off from the residuum, which consisted partly of a fine spongy earth, and partly of fragments of quartz. It was caught on a filter and sufficiently edulcorated. The last portion of edulcorating water dropped through the filter of an opalish hue.

The residuum, dried and exposed to a red heat, for ten minutes, $= \frac{1}{16}$ of a grain, $\frac{1}{16}$ of which consisted of fragments of quartz, $\frac{1}{32}$ was found to be silica, and $\frac{1}{32}$ alumina.

C.

(1.) The clear solution and the edulcorating water were poured into a large matrass and boiled, and whilst boiling, the contents were precipitated, in white flakes, by ammonia.

(2.) When the ammonia had ceased to produce any further precipitate, the clear fluid was decanted, and assayed with carbonate of ammonia. But its transparency was not in the least disturbed.

(3.) This clear fluid, together with the edulcorating water, with which the subsided precipitate had been washed, was gradually evaporated. When its volume was considerably diminished, a separation of a spongy earth took place, more copiously than I had reason to expect, and the quantity of it was still further increased by a few drops of ammonia. This earth, thus separated, was sufficiently edulcorated, and added to the former precipitate.

(4.) The fluid was again evaporated, and at last transferred to a crucible of platina, and the salt reduced to a dry state: on

redissolving this salt in distilled water a minute portion of earthy matter was separated, which, after edulcoration, was added to the rest. The fluid from which it had been separated, and the edulcorating water, were again evaporated to dryness, and the ammoniacal salt expelled by heat, in a platina crucible.

(5.) After the crucible had been made red hot, it was examined. I discovered on the bottom of it, some traces of earthy matter, and some spots, which had a glassy appearance. Water boiled upon it, dissolved nothing; from which circumstance, the absence of both of the fixed alkaline salts may be inferred. Neither did nitric acid produce any alteration. A few drops of sulphuric acid effected a solution of the substance, which adhered to the bottom of the crucible. Ammonia precipitated from it a small quantity of earth, which was transferred to the rest, and the sulphate of ammonia and edulcorating water were again evaporated and expelled by heat. A few spots of the same glazing still appeared. I had observed the same phænomenon in a former experiment: but in that, as well as in the present instance, the substance was in too small a quantity to become the subject of experiment.

D.

(1.) Upon the precipitate (C 1), and the earths collected at different times, whilst they were in a moist state, I poured a solution of potash in alcohol mixed with distilled water; in a short time, the greater part of it was dissolved.

The clear solution was decanted, and the undissolved sediment was transferred to a bason of pure silver, and boiled with a solution of potash.

(2.) When the potash ceased to act upon it, it was diluted

with distilled water and decanted from a brown powder, which had subsided. This powder edulcorated, dried, and ignited weighed $\frac{7}{12}$ of a grain; $\frac{1}{4}$ of a grain was alumina, $\frac{1}{32}$ silica, and $\frac{3}{32}$ oxide of iron.

E.

(1.) The solution effected by potash was decomposed and redissolved by muriatic acid, and the contents of the solution were precipitated by ammonia. The subsided precipitate was edulcorated.

(2.) The fluid and the edulcorating water were evaporated to dryness, and redissolved in distilled water. Here again, to my surprise, a separation took place of a white earth, more abundant than is usual in cases where ammonia is employed as a precipitant.

(3.) This earth and the precipitate were edulcorated with distilled water, until it ceased to affect a solution of nitrate of mercury. Collected, dried, and ignited, for one hour it weighed *whilst still warm* 32 $\frac{1}{12}$.

F.

(1.) This earth was placed in a crucible of platina, and repeatedly moistened with sulphuric acid, which was abstracted from it in the sand bath; distilled water effected the solution of the whole, except a white powder which weighed, after ignition, $2\frac{7}{32}$ grains. It was proved to be silica.

(2.) This solution was now mixed with some acetat of potash and gradually evaporated; large and regular crystals of alum were from time to time formed. A small portion of silica which weighed after ignition $\frac{1}{32}$ of grain was deposited;

some sulphat of lime also made its appearance, which washed with diluted alcohol and dried in a low heat $= \frac{7}{10}$ of a grain.

(3.) A portion of the fluid remained which neither the addition of potash nor the lapse of many weeks could induce to crystallize. Suspecting that it might contain glucine, I precipitated the contents by carbonat of ammonia, added to excess, and shook the mixture repeatedly and strongly. The precipitated earth was collected and the fluid boiled, but it was found to contain nothing but a minute portion of alumina.

(4.) The edulcorated earth was redissolved in sulphuric acid, except $\frac{1}{8}$ of a grain of ignited silica.

The solution was mixed with a little potash, and gradually evaporated. Sulphat of of lime was separated at several times and after long intervals, which sufficiently washed and dried in a low heat $= \frac{9}{32}$. Some silica also separated, but too minute in quantity to be ascertained by weight. The remaining fluid at length crystallized into regularly formed alum.

(5.) The whole, therefore, of the $38\frac{1}{16}$ (E. 3.) consisted of alumina except $9\frac{1}{8}$ of silica, and the lime contained in $\frac{1}{32}$ of sulphat of lime, which may be estimated about $\frac{1}{16}$ of a grain; the alumina, therefore, $= 29$; the alumina in B. and D. $= \frac{11}{32}$; the silica in B, D, and F, $= 3\frac{1}{16}$; the oxide of iron (D.) $= \frac{1}{32}$, and lime F, $\frac{1}{16}$; the volatile parts of this substance $= 15\frac{1}{8}$ in the 50 grains employed.

The sum total of these is - - - -	$47\frac{3}{16}$
Loss - - - - - -	$2\frac{13}{16}$
	50

I have subjected these crystals, as well as the harder species of this mineral, to analysis by means of direct solution in

sulphuric acid, and have found in each case the same fixed ingredients, viz. alumina, a small portion of silica, and a very minute quantity of lime. Both these latter ingredients are, I think, essential to the composition of this fossil, as I have always discovered them in the purest specimens. In this mode of analysis I experienced the same difficulty and tediousness of delay in bringing the last portions of the solution to crystallize into alum. This anomalous circumstance I have reason to attribute to a particular combination, which takes place between the sulphat of alumina and lime, silica, and potash. In my examination of the compact species there was no appearance of the sulphat of lime until the last; and in every experiment, previously to the fresh appearance of crystals of alum, that had been long delayed, silica and sulphat of lime were deposited.

I forbear entering into any further details concerning my former experiments on this curious fossil, as I have reason to think that it will still require a more particular and minute examination, on account of another ingredient which eluded my notice, and which may possibly impart to it its peculiar character. The scarcity of it has been hitherto a great bar to my experiments; I shall record, however, a few facts which I have lately observed, in the hope that at a future time I may be able to resume my examination of it.

I was induced to pay more attention to the volatile ingredients of this substance.* With this view, I introduced some of the crystals into a small retort, adapted a receiver unto it,

* Mr. HUMPHRY DAVY, whose well known skill and sagacity have probably rendered the researches of another person superfluous, had, I found, been engaged in the analysis of a mineral which is thought to be identical with the subject of these observations. He informed me that he had observed a peculiar smell, and acid properties in the water distilled from the substance which he examined.

and exposed the retort to a charcoal fire. The neck of the retort was soon covered with moisture, which passed into the receiver; and I observed a white crust gradually forming in the arch and neck of the retort.

On examination of the fluid in the receiver, it was found to have the same empyreumatic smell that I had observed before. It resembles very much the smell which that fluid is found to have which is distilled from the white crust that surrounds flint as a nucleus.

It changed litmus paper to a faint reddish hue. It produced no change on a solution of nitrat of silver, and scarcely a perceptible one, on that of nitrat of mercury.

The crust formed in the neck of the retort consisted of thin scales, which after the vessel had been dried, were disposed to separate from the glass in some places, but in others they firmly adhered unto it. They were opaque, like white enamel, and reflected the colours of the rainbow. A portion of this subtance exposed to the flame of the blow-pipe upon charcoal turned at first black, and then melted into a globule, that exhibited somewhat of a metallic splendor which soon grew dull. This substance is soluble in water; on evaporation of it, it assumes, at the edges of the fluid, a saline appearance, which, as the moisture evaporates, becomes earthy, opake, and white. Some of the solution changed litmus paper to a faint red. Lime and strontian waters produce in it white clouds, which a drop of nitric acid removes. Muriats of lime and barytes produce no change in it. Nitrat and acetat of barytes disturb its transparency, the effect produced by the latter is more evident. Nitrat of silver produces no effect, but nitrats of mercury and lead cause copious precipitates, which

are white, and soluble in nitric acid. Phosphat of ammonia and soda produced a white precipitate. Oxalat, tartrite, and prussiat of potash did not affect it, nor did sulphat of soda. Ammonia was dropped into it, but the fluid preserved its transparency. But carbonat of ammonia instantly caused a white precipitate, which was not redissolved by an excess of the precipitant: upon some of this subsided precipitate a concentrated solution of potash was poured and shaken with it, but it was not sensibly diminished. But if after edulcoration it be dissolved in nitric acid, and potash be added, no precipitate is produced.

Carbonat of potash causes a white precipitate when dropped into the aqueous solution of the scaly sublimate.

The supernatant fluid was poured off and gradually evaporated, but it became repeatedly turbid, nor could I by means either of the filter or alcohol prevent a recurrence of the same effect. Nearly the same result takes place when carbonat of ammonia is used as the precipitant.

Some of the white scales were moistened with sulphuric acid. No vapour arose.

Some of the precipitate obtained by means of carbonat of potash from the watery solution of this substance, was, after sufficient edulcoration, dissolved in sulphuric acid; the solution, on due evaporation, produced permanent crystals, some of which resembled alum, but others seemed to differ from it in external character. Ammonia decomposed the solution of them in water, and a few drops of liquid potash dissolved the precipitated earth. The quantity was too small for further experiment.

If distilled water be poured into the retort and boiled in it, so

as to dissolve what adheres to the neck and cavity of it, a further solution is effected, but differing in some measure from the solution of the sublimate collected from the neck of the vessel. This latter solution is found to contain lead. If nitric or muriatic acid be poured into the retort, so as to dissolve what *still* remains adhering to it, the presence of lead becomes more evident. Whence does this metal arise? I have reason to believe that it arises from the glass retort, which is corroded by the acid of the fossil extricated by heat. But what acid is it? It does not seem to be either the phosphoric or fluoric acids, the latter of which became the first object of my suspicion.

The opinion which Mr. DAVY suggested to me seems more probable, that it is of vegetable origin. Oxalic acid, on the authority of BERGMAN, may be volatilized; yet some of its properties are very extraordinary and do not accord with this idea.

I decomposed the watery solution of the scales by nitrat of lead, and after a sufficient edulcoration of the subsided precipitate, I dropped upon it some sulphuric acid. No fumes were perceptible. The sulphat of lead was separated by the filter, and the clear fluid, which passed through it, was gradually evaporated; small crystallizations were formed, the figure of which I could not ascertain; some of them were exposed to the flame of the blowpipe in a gold spoon; they did not burn to coal, nor give out any empyreumatic smell nor fuse, but they assumed an earthy appearance.*

* I subjected some of the Barnstaple mineral, with which Mr. RASHLEIGH kindly furnished me out of his cabinet, to experiment, with a view of ascertaining whether it would produce the same volatilized saline crust, as the Stepna Gwyn fossil, and I found that it did.

Uran-glimmer.

I shall add a few desultory remarks upon the yellow and green crystals, which frequently accompany this fossil.

I considered them to be the two species of Uran-glimmer, which had been examined by the celebrated KLAPROTH.

The yellow cubic crystals are light. Their specific gravity, taken at temperature 45° FAHRENHEIT, was 2,19.

Exposed to the flame of the blowpipe on charcoal, they decrepitate violently. A piece of this substance is taken up by phosphate of ammonia and soda, without effervescence, and communicates a light emerald-green colour to the fused globule.

By exposure to a red heat, this substance loses nearly a third part of its weight. It then becomes of a brassy colour.

It is soluble in the nitric and muriatic acids: but I could procure no crystallized salt from the solution of either of them.

By evaporation to dryness, and redissolving the mass, some silica is separated.

A.

(1.) A certain quantity of the yellow crystals were dissolved in nitric acid. Muriatic and sulphuric acids successively dropped into the solution produced no sensible change. The contents of the solution were precipitated by ammonia, in white clots, mixed with some of a yellowish hue. Ammonia, added in excess, betrayed no sign of the presence of copper.

(2.) The ammonia, on evaporation, was found to have held a portion of the mineral in solution. A fresh portion of ammonia dissolved more, but in a less quantity, at each succeeding affusion of it.

(3.) The precipitate, which had resisted the ammonia, was boiled in a silver crucible, with a solution of potash in alcohol,

diluted with distilled water, and a considerable portion of the substance was dissolved by it: the potash and the ammonia had dissolved rather more than half of the fixed ingredients of it.

(4.) The edulcorated residuum, which was of a dirty yellow colour, was transferred to a crucible of platina, and moistened with sulphuric acid, which was abstracted from it, in the sand-bath. The brownish-gray mass was elixated with distilled water, which dissolved nearly the whole of it. The residuum consisted of a white heavy powder, which, tried in different ways, was found to be sulphate of lead.

(5.) The solution effected by sulphuric acid was greenish. On evaporation, a salt was produced, of uncommon brilliancy, resembling scales of mica, or silver leaf. These diminished in quantity at every fresh solution and evaporation, and at last they could not be reproduced; but a confused crystallized mass remained. How far the platina crucible may have contributed to this phænomenon I cannot ascertain.

(6.) The solution of the saline mass was precipitated by potash, of a dark brown colour. The potash held nothing in solution. I redissolved the precipitate in nitric acid, and precipitated the solution by ammonia, of a bright yellow colour, peculiar to the oxide of uranium, with which it agreed in other properties.

(7.) What was dissolved by ammonia (2.) amounted to nearly $\frac{1}{2}$ part of the fixed ingredients. It was white, inclining to ash-colour. It tinged phosphate of soda and ammonia of a light green. It was soluble in sulphuric acid, except a few gelatinous flakes. The solution was greenish; gradually evaporated, it shot into a number of minute stellated crystallizations, which were circular, and consisted of rays diverging from a

centre. They were, in general, colourless: a few of them were tinged of a smoke-colour. They soon became deliquescent. Upon evaporation, the same crystallizations were produced. After a time, some detached, regular, and permanent crystals were formed, which were colourless. Their figure I could not accurately ascertain. They were exposed to a red heat in a platina crucible. No ammoniacal vapour was perceptible. The crystals melted into opaque globules: some of these were transferred to a small glass, and distilled water was poured upon them. No solution took place, apparently: on shaking the glass, the globules fell to pieces into gelatinous flakes, which were white. Some of the supernatant fluid was tried with muriate of barytes, which produced a cloud. But neither ammonia nor prussiate of potash caused any change in it. It is soluble also in nitric acid: the solution formed a confused crystallized mass, which soon became deliquescent. Zinc, immersed in it, caused the separation of white gelatinous flakes. Iron caused no change. Ammonia and potash threw down white precipitates, a portion of which were redissolved. The carbonates of soda, potash, and ammonia produced white precipitates. Prussiate of potash threw down the contents of the solution in distinct flakes, of the colour of mahogany; and the solution of galls in alcohol caused a light yellow powder to subside. It is soluble also in muriatic acid; the solution is a very dilute green. It requires an excess of acid to hold the substance in solution; which, after a time, deposits crystalline grains of a yellowish colour, which require a large quantity of water to dissolve them.

Acetic acid does not dissolve this powder.

(8.) What was dissolved by potash (3.) was of an ISABELLA colour: it was tried with nitric, muriatic, and sulphuric acids,

neither of which could dissolve the whole of it. What resisted the two former acids was found to be silica. That which remained undissolved by the latter, was silica and sulphate of lead. Evaporation of the latter solution, betrayed also the presence of lime, in the state of sulphate. The nitric and muriatic solutions, on evaporation, deposited nitrate and muriate of lead; and sulphuric acid dropped into them produced a small quantity of sulphate of lime.

The nitrate and muriate of lead were decomposed by sulphuric acid, and the lead reduced on charcoal.

Ammonia precipitated what remained in these solutions, and redissolved a part of the precipitates, which agreed in properties with that substance before mentioned (2.); the remainder was of a brighter yellow. But I could not bring the solution of it in nitric acid to crystallize.

B.

(1.) Some of the yellow crystals, which had not the slightest appearance of being contaminated with extraneous matter, were dissolved in sulphuric acid. Silica was separated; and the presence of lime and lead proved by the appearance of their respective sulphates.

(2.) If sulphate of ammonia is dropped into a solution of this mineral in nitric or muriatic acids, no change takes place, *immediately*. But on evaporation, a yellowish crust is deposited, which is insoluble in water. A solution of carbonate of soda in water, boiled on it, becomes yellowish-brown, and the greater part of it is dissolved. The residuum, which is white, is reduced on charcoal to a globule of lead. What the carbonate of soda had dissolved was found to be oxide of uranium. Sulphuric acid *alone*, does not produce this deposited crust.

(3.) Some perfectly pure crystals were dissolved in muriatic acid. Some silica was separated. A few drops of sulphuric acid were dropped into the solution, which produced no immediate change: on evaporation a white powder separated, which consisted in part of sulphate of lime. The remainder, exposed to the flame of the blowpipe, was reduced to globules of lead.

The solution was decomposed by ammonia, which redissolved a part of the precipitate; and, after edulcoration, the precipitate was dissolved by nitric acid, and precipitated again by ammonia, which held a less quantity in solution. The edulcorated precipitate was now boiled with a solution of carbonate of soda, which dissolved a large portion of it. The solution was yellowish-brown, and contained oxide of uranium. What was undissolved by the carbonate of soda was dissolved in sulphuric acid, and seemed to be the same substance as that which the ammonia held in solution. A. (2.)

The scarcity of this beautiful mineral has precluded me from operating on such a sufficient quantity, as a regular and rigid analysis required.

The substance, which is held in solution by ammonia, has some peculiar properties that seem to distinguish it from uranium. And if this mineral be the Uran-glimmer, I have certainly detected the oxide of lead, lime, and silica in it, which have not hitherto been considered as ingredients of that fossil. The green crystals differ in no respect from the yellow, except in containing a little of the oxide of copper.

Creed,
June 14th, 1803.

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